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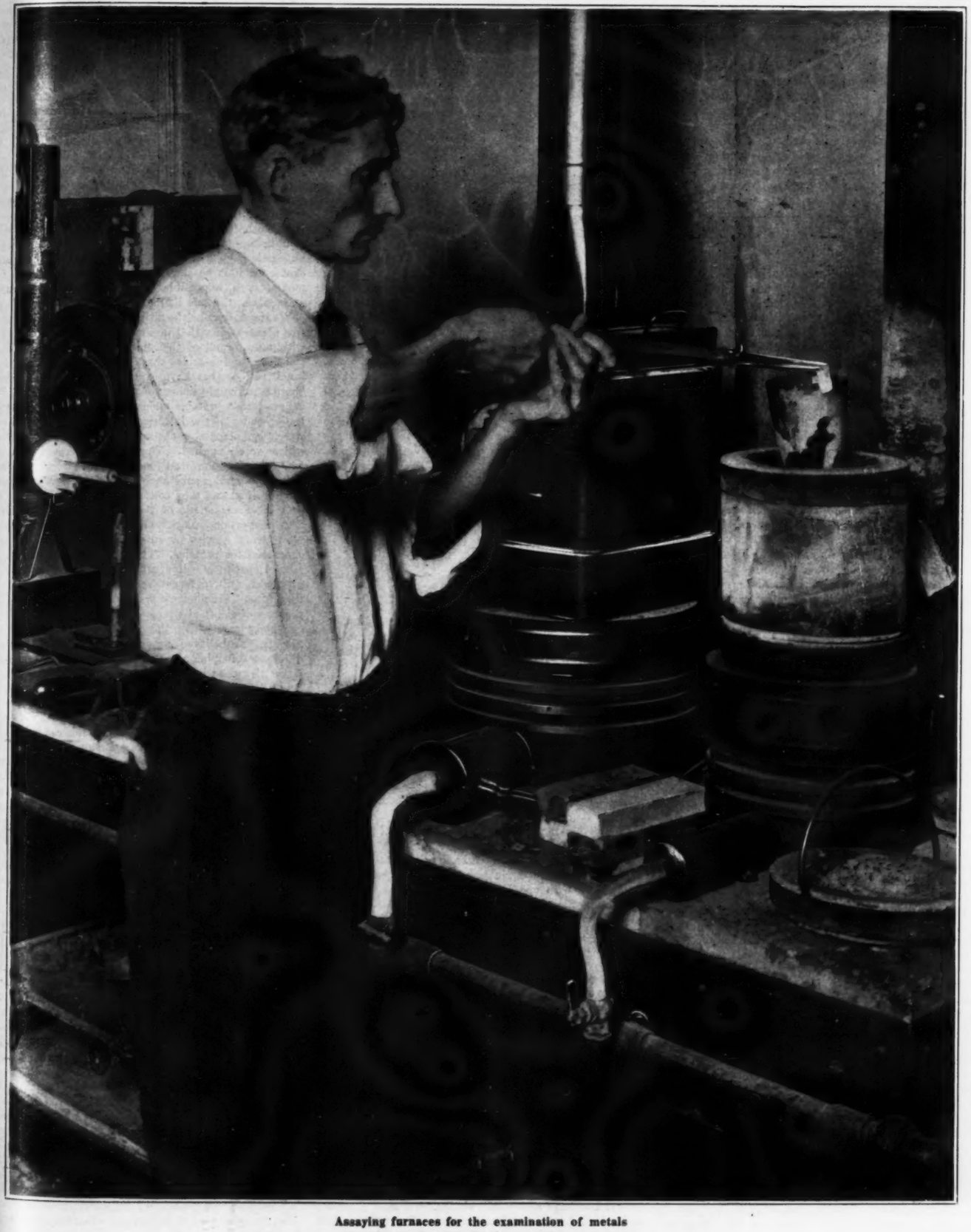


Assaying furnaces for the examination of metals

HOW THE GOVERNMENT PROTECTS ITS REVENUES [See page 184]

The Biochemical Analysis of Nutrition*

A Subject of Great Importance in Relation to Our Food Supplies

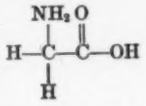
Carl L. Alsberg, U. S. Bureau of Chemistry

THERE are a number of ways in which nutrition may be studied. By the statistical method the effect of diet upon definite social or geographical groups of individuals or upon inmates of hospitals, asylums, or barracks is determined. The physical method determines the energy income and outgo of the individual. The physiological method determines the rôle in nutrition of individual organs. Nutrition may, furthermore, be studied by the method of biochemical analysis. This method seeks to follow each one of the many chemical complexes that enter into the composition of food in its course through the animal organism. Therefore, for the purposes of this method the component chemical radicals of the food must be known. This information can be obtained only by resolving the food elements into their component parts, that is, by analyzing them biochemically. This paper, therefore, presents a discussion of some of the component parts of the food elements and of the fate in the metabolism of some of the individual chemical complexes that are found free or combined in food, in so far as their fate is understood or surmised.

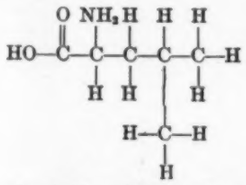
My reason for selecting this particular subject is that during recent years perhaps the most interesting contributions to knowledge made by biochemists have been in this field. Among the most important contributions of recent years to the study of nutrition are investigations upon the rôle of the proteins and of the nitrogenous constituents of the food in the animal organism. You know, no doubt, that in Liebig's time proteins were regarded as that element of the food which supplied the material for growth, tissue maintenance, and repair, as well as for most of the energy. Though it was soon demonstrated that while proteins did and could furnish energy, under ordinary conditions this was supplied in the main by sugar and other carbohydrates and by catabolized fats. The proteins were, however, still regarded as all of about equal value, and their value was estimated as proportional to the amount of nitrogen they supplied to the animal organism. One protein was regarded as of about as much dietary value as another. This was, however, soon found to be an erroneous notion. It was learned that with certain proteins, gelatine for example, as the sole source of nitrogen in the diet, life could be not supported. The proteins then came to be divided into two classes, the true proteins and the albuminoids. Gelatine was classed as an albuminoid. The fact that it and some other proteins were found to be incapable of supporting life was regarded as corroborative evidence that they were not true proteins. Why they are incapable of supporting life was not known. It was known that animals could not live without proteins in the diet, but it was believed that relatively few of the proteins were incapable of supplying all the nitrogenous needs of the animal organism.

That is about where matters rested for a long time. Then in 1901 Loewi published a most startling investigation, so startling in fact that it received scant attention. He subjected the pancreatic gland, which all of you know as sweet-bread, to self-digestion, the process that is technically known as autolysis. As you know, practically all cells and tissues contain many enzymes or ferments, some of them similar to those secreted in the stomach and intestines for the purpose of digesting proteins. Therefore, under suitable conditions tissues can be made to digest themselves. Loewi caused the self-digestion of the pancreas to proceed until the digestion of the proteins in the gland was so complete that no trace of any reaction for protein could be obtained. This predigested material as the sole nitrogenous element of the diet was then fed to dogs and it was found that upon such a diet the animals maintained their weight. These results were remarkable because they tended to show that it was not absolutely necessary to life that protein be an element of the diet. For a time these findings were ignored, but it has since been shown that life can be supported for a time at least upon a diet containing no complexes known as protein, but instead a suitable mixture of amino-acids.

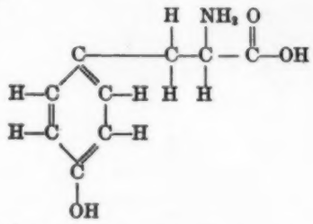
For the information of those of you who have not followed the chemistry of the proteins during recent years I may say that the proteins are combinations of the amino-acids, by which we mean ordinary organic acids in which one or two hydrogen atoms have been replaced by the amino group, NH_2. The simplest amino-acid found in protein is glycerine which, as you see, is derived from acetic acid.



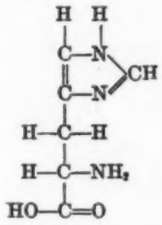
There are seventeen of these amino-acids commonly found in proteins. Several others have been reported more or less definitely. Perhaps the commonest one is leucine, which has six carbon atoms.



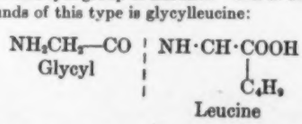
As you see, in all these substances the amino group is associated with the carbon atom adjacent to a carboxyl, COOH, group, the alpha position, as it is known technically. When two amino groups are present one is in the alpha position; the other is associated with the terminal carbon atom at the other end of the chain, the omega position. Lysine is such a diamino-acid, as you can observe from the formula $CH_2(NH_2)(CH_2)_3$. CH $(NH_2)COOH$. In these acids the atoms are arranged in a chain. However, the protein molecule may contain amino-acids in which the arrangement is such that a ring is found. Tyrosine, a benzol derivative, is a good example.



Histidine is another ring compound:



Tryptophane, which is related to indigo, is still another. The proteins are combinations of a number of these amino-acids with one another. Most of the seventeen or eighteen amino-acids are found in each protein. In all probability each molecule of protein contains a number of molecules of a given amino-acid, so that the protein molecule may be very large. The union of the amino-acids with one another is, so far as now known, always of the same kind; the amino group of one acid is united with the carboyxl group of another. One of the simplest compounds of this type is glycylleucine:



These compounds are known as peptides and were first made artificially. They were later discovered among the decomposition products of proteins. A peptide with a molecular weight of more than one thousand, composed of eighteen molecules of amino-acids, has been made, Substances with still larger molecular weights could be made, were it worth while. Thus substances, which are believed to have a structure similar to that of proteins, and which have a molecule approaching in size that of some proteins, have actually been made.

After this digression in explanation of the chemical structure of proteins let us return to the problem of the nutrition of animals with amino-acids. Loewi found that animal could be kept for a time without loss in weight upon a mixture of completely digested protein. It was not at that time known that such mixtures contained peptides, which, as I have explained, have certain resemblances to proteins. It was therefore a most important discovery when it was later determined that an animal could be maintained for a time upon an artificial mixture of the pure crystalline seventeen or eighteen amino-acids found in proteins free from peptides. If these observations are correct, it is theoretically possible to supply the so-called protein needs of animals by wholly artificial substances.

Certain investigators have very recently gone still further by endeavoring to show that some of the nitrogenous needs of the animal organism can be supplied by simple salts of ammonia such as are used in fertilizers. It has hitherto been believed that only plants are capable of utilizing ammonia. The matter has not been settled; but there is good reason to believe, as will appear later, that even if it be shown that animals can utilize ammonia, it is impossible to support animal life upon ammonia as the sole source of nitrogen.

Whatever may be the ultimate practical significance of the observations that animals can supply themselves with most or all of their nitrogen needs by means of synthetic amino-acids, these experiments have led to investigations that have explained much that has been obscure in the physiology of nutrition. Formerly it was believed that proteins when ingested were digested by the enzymes of the intestinal tract and converted into simpler substances, in the main albumoses and peptones, which were absorbed. These albumoses and peptones, while simpler than most food proteins, are, nevertheless, still very complex substances. It was believed that they are absorbed and then converted by the animal into the protein characteristic of that particular animal. How that conversion was accomplished was not understood. Now every species of animal and plant has its own characteristic proteins. The proteins of even closely related species are different. The proteins of the food supply are quite different from those of the animal taking that food. Much work was done to explain how the proteins of the food were converted into the proteins of the body and where this conversion took place. At first it was believed to occur in the blood. Later a difference of opinion arose as to whether it took place in the tissues or in the intestinal wall. As food proteins could be demonstrated in neither place, the matter remained unsettled. We know to-day that neither hypothesis is tenable. Proteins are not ordinarily absorbed as such. They are completed dismembered within the intestinal canal into their component amino-acids and these are absorbed. As long as it was not known that an animal can be maintained upon pure synthetic amino-acids, no one had any reason to believe that proteins were completely digested before absorption.

Now what happens to these amino-acids after they are absorbed? As ordinary diets may contain more nitrogenous material than is needed by the organism, a part of the amino-acids is changed within the walls of the intestinal canal by the removal of the amino group to form ammonia. As this takes place in the presence of carbonic acid, ammonium carbonate and ammonium carbamate are formed. It has recently been found that there is an equilibrium between these two substances, so that where one is present in solution there is also found a definite amount of the other. It is an easy step from ammonium carbamate to urea. Thus the amino group split off from the amino-acid in the intestinal wall or elsewhere is ultimately converted into urea and excreted. There are probably other methods of the formation of urea, as, for example, by cleavage from arginine which contains a guanidine grouping closely related to urea. After the removal of the amino group from the amino-acids there is left a carbonaceous residue which may be burned to furnish energy, perhaps directly, perhaps after conversion into sugar. A portion of the amino-acids absorbed by the intestines is not, however, deprived of its nitrogen, but passes into the blood stream from which it is absorbed by each individual cell according to that cell's particular needs. The cell then reconstructs from these amino-acids its own characteristic protein. Thus it is possible to explain in a comparatively simple manner how, for example, wheat protein when fed to an animal is converted into the characteristic proteins of that

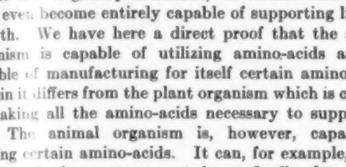
*The address of the retiring President of the Chemical Society of Washington, given at a joint meeting of that Society with the Washington Academy of Sciences, January 13, 1916 From the Journal of the Society.

animal. It is done by the cells of the tissues from amino-acids supplied to the cells by the blood, the blood receiving the amino-acids from the intestinal wall.

This theory concerning the fate of food proteins in the animal body is supported by certain very interesting feeding experiments. In these experiments animals, usually white rats, were fed upon definite mixtures containing only a few pure food substances. These mixtures consisted of sugar, fat, mineral salts, and a single pure protein. In each set of experiments a different pure protein was used in each series, all other factors remaining constant. It is in this way possible to determine the nutritive value of individual proteins. The results of this work indicate that many proteins are incapable either of supporting life or of producing growth. On the whole it may be said that many more vegetables than animal proteins are defective in this way. Now, when the composition of such defective proteins is compared with that of proteins that are not defective in this respect, it is found that the defective proteins lack one or more of the amino-acids which are found in the proteins that are not defective. This is very much oftener true for the vegetable proteins than for the animal proteins. Some lack lysine, others tryptophane or histidine, or cystine. The latter is an amino-acid containing sulphur, the usual form in which sulphur is contained in proteins. Some proteins lack more than one amino-acid. Gelatine, for example, contains no cystine, tyrosine, or tryptophane. Now it has been shown in certain cases that if to a diet of the kind just described, containing a single defective protein, there be added the amino-acids which that protein lacks, the value of the diet is greatly increased; in certain instances it may even become entirely capable of supporting life and growth. We have here a direct proof that the animal organism is capable of utilizing amino-acids and incapable of manufacturing for itself certain amino-acids. Herein it differs from the plant organism which is capable of making all the amino-acids necessary to support its life. The animal organism is, however, capable of making certain amino-acids. It can, for example, make glycine. It has not as yet been finally determined exactly which amino-acids can be made by animals and which can not.

There are a number of ways in which the lack of certain amino-acids may affect the functioning of the animal organism. Their lack may, of course, make it impossible for the animal to manufacture its own tissue protein. It suffers a kind of starvation. There are, however, more indirect ways in which the absence from the diet of a necessary amino-acid may be important. It has recently been found that the iodine compound of the thyroid gland, the gland that you feel in the neck about the Adam's apple, is a derivative of the amino-acid tryptophane. It has long been known that the normal functioning of the thyroid gland is essential to life and health. It has been found that the normally functioning gland contains the iodine compound now believed to be formed from tryptophane. It is therefore possible that when there is no tryptophane in the diet, difficulty in the formation of the iodine compound necessary for the thyroid gland results with corresponding disturbance of the gland's function.

The iodine compound of the thyroid gland is physiologically active; that is to say, it is poisonous. It is not, however, the only physiologically active substance produced from amino-acids in the animal metabolism. Adrenaline



is another such substance probably derived from an amino-acid. As you may see by comparing the formulæ, it is related to the amino-acid tyrosine. It is formed in the adrenal glands, two small glands found in the kidney fat just above the kidneys. Hence their name, which was given to them before it was known that they have no direct relation to the kidneys. They apparently furnish adrenaline to the blood. Adrenaline when injected into the blood causes the small blood vessels to contract and therefore the blood pressure to rise, since the heart then pumps against the increased resistance of the contracted vessels. When the adrenal glands do not function normally, as for example when they are tuberculous, Addison's disease develops, which is characterized among other symptoms by a low blood pressure. The substance adrenaline is probably known to you, since it is used therapeutically in a number of ways, for instance, to constrict capillary blood vessels to stop bleeding from the capillaries on wound surfaces. If there is an absence from the diet of the material necessary to form adrenaline, it is conceivable that symptoms other than those of starvation might result.

There is a small gland at the base of the brain known as the pituitary gland, which is apparently necessary to life and which manufactures a physiologically active substance, probably derived from the amino-acid histidine. Disease of this gland seems to occur in certain giants and in the disease akromegaly, in which among other symptoms is found enlargement of the bones of the face and the extremities.

A very interesting physiologically active substance, para oxyphenylethylamine, is formed in the self-digestion in the pancreas from tyrosine, as shown by Emerson. It is tyrosine from which carbonic acid has been removed. This is of great interest, since it was at one time believed that carbonic acid was formed in living organisms solely by oxidation. This observation shows that it can also be formed by enzymatic cleavage. This is of profound biological interest, since in all probability the energy which is required by organisms living in an environment free from oxygen is obtained by reactions of this type. Ordinarily we think of such organisms as being limited to the fungi, but there are quite highly organized animals which live in this way; for example, parasitic intestinal worms. The gases of the intestines are practically free from oxygen. In all probability these worms obtain the energy necessary for the maintenance of life by cleavages, rather than by direct oxidation. Finally, it is interesting to note that most of these physiologically active substances are amines, probably derived from amino-acids by removal of carbonic acid, that is, by the elimination of the carboxyl, COOH. This method of the formation of amines is probably quite common in plants and leads to the formation of various poisonous plant bases. It has been suggested that the active principle of ergot is formed in this way.

In making this digression to explain that physiologically active substances may be formed from amino-acids, it was my purpose to suggest that the defective or incomplete proteins, when the main or sole nitrogenous element of the diet, may not merely produce a form of starvation, but that they may also have an indirect action through failure to supply the raw materials which are needed by certain glands to elaborate their specific products.

It should not be inferred from what has been said that these incomplete or defective proteins are without food value. On the contrary they may be of great food value. They are merely not of themselves sufficient, but they can be made sufficient if supplemented in relatively small amounts by other proteins that contain the lacking elements. This is a matter of the greatest practical importance in the feeding of farm animals. Animal feed consist in the main of vegetable proteins which are often incomplete. Obviously such a feed can be used most economically if it is supplemented by small amounts of proteins which supply the missing elements. To do this it is necessary to know what amino-acids are contained in the different vegetable foods. Agricultural chemists are now engaged in studying this problem. In this connection it must be pointed out that there is a distinction between maintenance and growth. It is perfectly possible to maintain an animal in good health upon a diet which will not permit it to grow. It is possible to stop the growth of an animal by putting it upon such a diet. When the diet is changed growth may be resumed. The capacity for growth has not been destroyed. Furthermore, it has been shown that it is not safe to conclude that a given diet is sufficient if it permits an animal to develop to adult life. It is necessary to show that it will also permit it to reproduce. There are diets that apparently permit perfect development but do not permit reproduction. It has also been found that more growth will take place upon a diet containing a mixture of incomplete proteins than upon a diet containing only one or two of them. Thus growing pigs will utilize for growth only about 24 per cent of the proteins of corn or wheat, whereas upon a mixture of the two grains they will utilize for growth about 33 per cent. It may be stated by way of comparison that pigs will utilize for growth about 60 per cent of the milk proteins fed.

Some of the investigators who put animals upon these restricted diets found that the animals throve a great deal better if small quantities of certain substances were added to the diet. The presence of so small a quantity of milk that its protein was a negligible factor kept animals growing or in good condition upon diets that would not otherwise permit the animals to remain in good health. A diet which does not permit normal growth will do so if a small quantity of butter is added. Apparently there are present in certain foods small quantities of substances of unknown nature which are necessary to growth and life. What this substance in butter is is not known. It seems to contain neither nitrogen nor phosphorus. This is of importance, since it proves that the substance is not a lipoid. Lipoids are complex fat-like substances containing both nitrogen and phosphorus. It has been claimed that a diet without lipoids will not support life. The absence of nitrogen from the growth-promoting substance of butter indicates that it is not related to the vitamine of rice which is almost certainly a nitrogenous base. The vitamines are substances of unknown composition, found in certain foods and essential to life. In the polishing of rice the vitamine is removed. An individual living upon a diet of polished rice will develop a disease known as beri-beri, frequently found among rice eaters. There is apparently a whole series of substances found in foods which are necessary to life. The absence of certain of them is believed to produce scurvy. A similar hypothesis has been advanced concerning the disease pellagra.

Little is known as yet concerning the chemical nature of these substances. They are very unstable, being decomposed by heating, especially by sterilizing under pressure. Lime juice has been used for many years as an antiscorbutic. It is not particualrly rich in these substances, but in lime juice they seem unusually stable. It has been suggested that the free organic acid present in the lime juice protects the antiscorbutic substances.

It is therefore quite evident that the last decade has brought forward many contributions of the greatest hygienic and economic importance. It is quite certain that more important discoveries are still to come. It is therefore well to look askance at popular prophecies concerning the approaching inadequacy of the world's food supply. When our viewpoint has changed so radically in a few years, it is idle to speculate about the future. If I have been able tonight to present this point to you clearly, I have accomplished my purpose.

New Methods for the Purification of Colloidal Solutions

THE method depends on ultra-filtration by the use of a collodion membrane. The apparatus consists of a funnel-shaped vessel containing the colloidal solution to be purified, this vessel being closed near the top by the ultra-filter, while water under pressure may be introduced through the lower narrow end of the funnel, the rate of flow being regulated by means of a tap. The aqueous solution which passes through the collodion filter is run off into collecting vessels. It is essential that the flow of liquid should be upwards, for preliminary experiments in which the colloidal solution was placed above the ultra-filter showed that the colloidal particles, under the continued influence of gravity and of the current of liquid, tended to accumulate on the upper surface of the membrane to such an extent that this rapidly became impermeable to the wash water. For the production of suitable filters, the author recommends the use of solutions of nitrocellulose in acetic acid. The solution, of from 7-5 to 15 per cent strength, is spread out in a uniform, thin layer on a glass plate, and the film subjected to the action of running water for five to ten minutes. The porosity varies very considerably with the concentration of the collodion, a membrane prepared from a 7.5 per cent solution having been found to be about ten times as porous as that prepared from a 15-per cent solution. The membranes may not be dried, and even when kept under water it is found that aging takes place. The apparatus has been found to give satisfactory results in the purification of colloidal solutions of antimony, arsenic, and molybednum sulphides, gold, palladium, silicic acid, ferric hydroxide, aluminium hydroxide, vanadium pentoxide, blood serum, and egg-albumin. The apparatus may also be employed for the preparation of colloidal solutions containing particles of determinate size, for the concentration of dilute solutions of colloids, for measurements of the size of colloidal particles, and for the purification of precipitated substances which cannot be easily washed by the usual filtration methods.—G. WEGELIN in "Kollard Zeits," from a note in the *Journal of the Society of Chemical Industry*.

New Methods in the Wheat-Starch Industry

MODERN methods of starch manufacture enable a larger yield to be obtained from the flour than was possible with the older methods, this being attained by a more perfect separation of the starch from the gluten. The following method gives a yield of 58-60 per cent of starch as compared with 34-52 per cent by the old methods. The flour is kneeded into a dough with water and washed in an extractor; the starch-milk obtained is passed through the usual sieves and the fine starch then separated by centrifugal action. The portion of the starch still remaining mixed with the gluten on the sieves is separated by subjecting the gluten to fermentation or treatment with dilute ammonia and acetic acid, and centrifuging the mixture. The gluten and a small quantity of bran separated in the process may be used as feeding stuffs.—C. GOLDBECK in Chem. Zeit., from a note in the *Journal of the Society of Chemical Industry*.

How Eclipses Occur—II

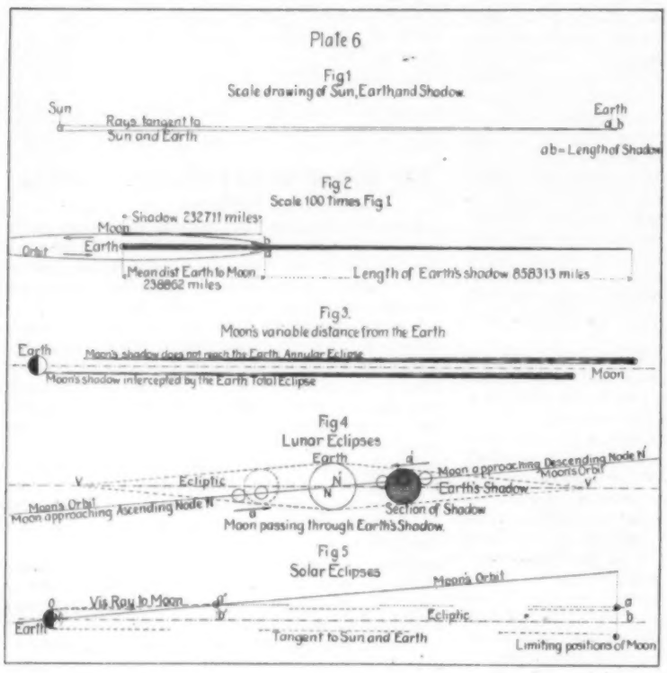
And the Solar and Lunar Eclipses in 1917-18

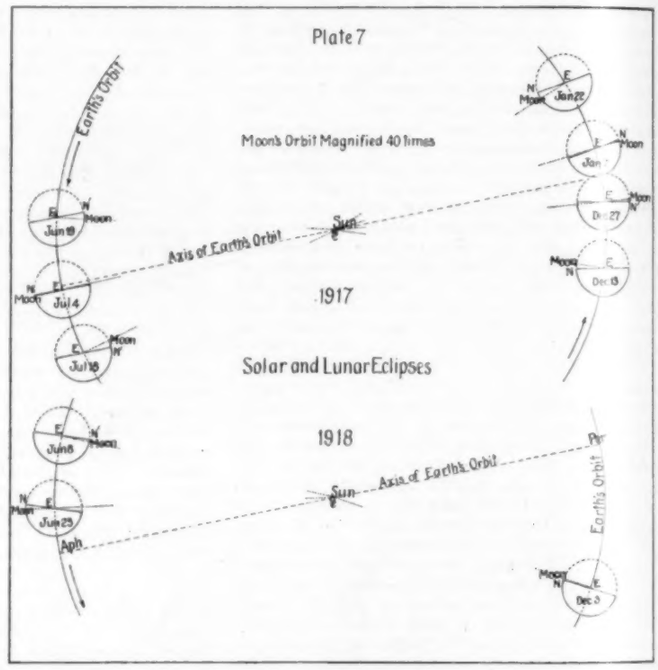
Concluded from SCIENTIFIC AMERICAN SUPPLEMENT No. 2150, Page 166, March 17, 1917

SOLAR AND LUNAR ECLIPSES, 1917-18

The unusually large number of eclipses included in the year 1917, is due to their occurrence at the beginning, the middle, and the end of the year. If the line of nodes of the moon's orbit moved into parallel positions eclipses would occur at average intervals of six months. But on account of the regression of the nodes the interval is less than six months, making the dates for this year possible. In the present position of the moon's orbit eclipses occur in December and January; and also in June and July. The last date in 1916 was that of an eclipse of the sun on December 24th.

Plate 7. Since eclipses occur only during the first sixth, seventh and last months, it is unnecessary to plot the whole of the earth's orbit. The moon's orbit is magnified in order to render visible the moon's position at the date of an eclipse. Since the angle between the earth's radius vector and the line of nodes is not affected by magnification, if the plot of the orbits of the earth and moon is carefully made, it is possible to reach conclusions in regard to an eclipse by a simple inspection of the drawing. The dates are given in Greenwich mean time.





1917

A total eclipse of the moon, January $7^d.82$. The moon is above the ecliptic and approaching the descending node N^1. The plot shows that it is very near the ecliptic. It is therefore wholly immersed in the earth's shadow, passing through its upper part.

A partial eclipse of the sun, January $22^d.81$. The moon is above the ecliptic and has recently left the ascending node N. Its distance above the ecliptic indicates the eclipse is visible in the northern hemisphere.

A partial eclipse of the sun, June $19^d.05$. The moon is above the ecliptic and approaching the descending node N^1. The distance above the ecliptic combined with the earth's position, which is very near that of the summer solstice when the north polar circle is exposed to the sun's rays, indicates that the eclipse is visible in the polar regions.

A total eclipse of the moon, July $4^d.40$. The moon has left the ascending node N. It is above the ecliptic and very near the node. It is therefore totally immersed in the earth's shadow.

A partial eclipse of the sun, July $18^d.61$. The moon is below the ecliptic and has recently left the descending node N^1. The eclipse is therefore visible in the southern hemisphere.

An annular eclipse of the sun, December $13^d.9$. The moon is approaching the ascending node N, and is below the ecliptic. The earth is within a few days of the winter solstice, and the eclipse is visible in the south polar regions.

A total eclipse of the moon, December $27^d.9$. The moon is leaving the descending node N^1 and is below the ecliptic. It passes through the lower section of the shadow.

1918

A total eclipse of the sun, June $8^d.42$. The moon is approaching the descending node N^1 and is above the ecliptic. The date is within two weeks of that of the summer solstice. The eclipse is therefore visible in the northern hemisphere.

A partial eclipse of the moon, June $23^d.95$. The moon has passed the ascending node N, and is above the ecliptic.

On account of its great distance from the ecliptic the eclipse is partial. Compare the distance from the node with the distances at the dates of total eclipse.

An annular eclipse of the sun, December $3^d.14$. The moon is approaching the ascending node N. It is below and near the ecliptic. Taking into consideration the position of the earth in its orbit, the eclipse is visible in the southern hemisphere.

Plate 8. The diameter of the earth's shadow at the section through which the moon passes when the earth is at its mean distance from the sun, and the moon at its mean distance from the earth, is about 5,800 miles, if an allowance is made for the earth's atmosphere. This is nearly 2.7 times the moon's diameter. Since the moon moves in one hour a distance which is about equal to its diameter, it is a simple matter to determine the duration of an eclipse.

Penumbra Fig. 2.—Let S and E represent respectively an illuminating body and one casting a shadow. The tangents bv and cv determine the shadow outline; and the tangents ba and cd the penumbral area. If from any point o within it os be drawn tangent to E it will intersect S at s, and to an observer at o, S would be partially eclipsed. The light is therefore diminished within this area. Fig. 1 shows the moon passing through the earth's shadow and penumbra projected on the plane of the ecliptic.

The conditions which result in the ten eclipses in 1917-18 have already been considered. The following examples of solar and lunar eclipses are selected for more detailed examination.

TOTAL AND PARTIAL ECLIPSES OF THE MOON

Plate 8, Fig. 3. A total eclipse January $7^d.82$, 1917, as seen from the earth. Referring again to the positions of the earth and moon at this date in Plate 7, the earth's radius vector forms a small angle with the line of nodes. Since the axis of the shadow is a continuation of the earth's radius vector, if these lines coincided the axis of the shadow would be perpendicular to the plain of the paper, and the apparent, or projected length of the axis would be reduced to nothing, *i. e.*, the point N^1. In that case the moon's center would cross the shadow along a diameter of the section, *i. e.*, centrally—the maximum possible path. In the figure the projection of the axis on the plane of the paper foreshortens it to about 1/25 of its length; and the distance from N^1 to the center of the section of the shadow where the moon crosses it is reduced in the same proportion. The close proximity of the shadow section to N^1 results in a total eclipse, the moon passing through the upper part of the shadow.

Fig. 4. Partial Eclipse, June $23^d.95$, 1918. The angular distance of the moon from the node N is a little over four times that of Fig. 3. The foreshortened length of the shadow, which is extended beyond the limits of the plate, is about 1/6 of its actual length; and the section is at a distance from the node a little over four times that of Fig. 3. As a consequence the moon is only partially eclipsed. A small increase in the distance from the node would result in no eclipse.

TOTAL AND ANNULAR ECLIPSES OF THE SUN

Fig. 5. An Annular Eclipse, December $13^d.9$, 1917. The moon is at a distance from the node which brings it so far below the ecliptic that the axis of the shadow produced, pierces the earth's surface at the south pole. The length of the shadow is not great enough to reach the earth, and the result is an annular eclipse. The date is within a week of that of the winter solstice which explains the earth's position and the exposure of the south polar regions to the sun's rays. The earth is within 18 days of perihelion when the moon's shadow is approaching its minimum length.

Fig. 6. A Total Eclipse January $8^d.42$, 1918. The moon's distance from the node shows that the axis of its shadow will pierce the earth's surface above the ecliptic. The date is within two weeks of that of the summer solstice. It is also within 26 days of aphelion when the moon's shadow is approaching its maximum length. Moreover, the moon's position in its orbit is very much nearer perigee than apogee. As a consequence the shadow is intercepted by the earth.

The penumbral area on the earth's surface is that portion within which a partial eclipse is observed. In

Fig. 2 if the body E is the moon and S the sun, the tangents ba and cd produced will intersect the earth's surface in the limit of the penumbral area.

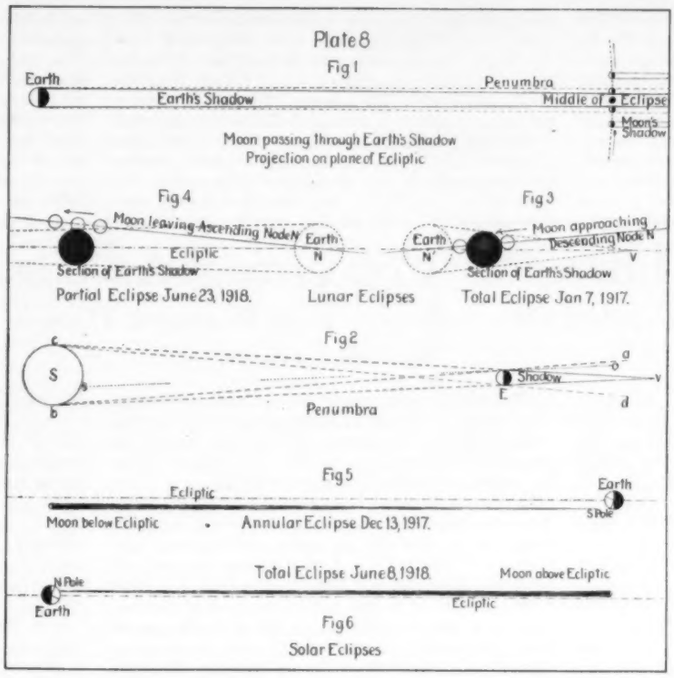
Plate 9, Fig. 1, shows the path of the moon's shadow in the total eclipse of June 8th, 1918. It will cross North America from northwest to southeast. Since the earth will be within less than four weeks of alphelion, the sun's apparent diameter will approach the minimum. The moon's position will be within four days of perigee, and will subtend an angle greater than its mean value. The result will be a total eclipse (Fig. 3), which will be visible to an observer situated within the shadow area.

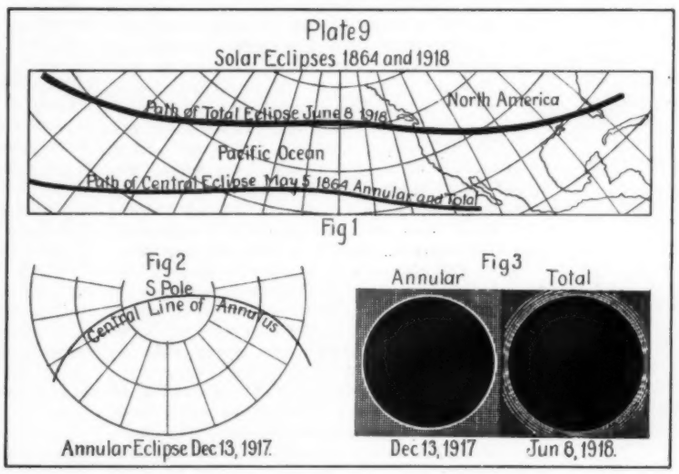
Fig. 2 represents the south polar regions and the central line of the annulus which passes through the south pole. Along this path the annular eclipse of December 13th, 1917, will be visible. At this date the earth will be within 18 days of perihelion and the sun's apparent diameter will approach a maximum. While the moon's position will be nearer perigee than apogee, the sun's apparent diameter will be greater than that of the moon. The result will be an annular eclipse, Fig. 3.

The Major and Minor limits. The angle of inclination of the moon's orbit is variable within narrow limits. If the moon's distance from the ecliptic in Fig. 5, Plate 8, were increased a little the central line of the shadow would fall clear of the earth. In this particular case the moon's distance fron the node is about 9°.5 of the moon's orbit. It is evident that if the angle of inclination of the orbit were smaller an eclipse would be possible if the moon were at a greater distance from the node. It is found that the maximum possible distance from the node for a central eclipse is 11°.8, the major limit. If the distance is less than 9°.9, the minor limit, a central eclipse is certain. The major and minor limits for a partial eclipse are respectively 18°.5 and 15°.35.

In a lunar eclipse the major limit is 12°.25. At this distance from the node an eclipse may occur. Within the minor limit 9°.5 an eclipse is certain.

A lunar eclipse is in general possible only twice a year, *i. e.*, only once during the eclipse month. Whatever may be the position of the moon at the date of an





eclipse, a month later it will be too far from the node for an eclipse to be possible. It should be noted, however, that there is one exception, viz., when there are three eclipse months, at the beginning, the middle, and the end of the year. The years 1898 and 1917 are the only exceptions which appear in the accompanying table.

RECURRENCE OF ECLIPSES

The Sidereal and Synodic Periods.—The moon completes one revolution on the average in nearly 27 ⅓ days (=27^d.32166) that is if the time be computed by reference to a fixed star which would be practically the same as if the earth were stationary. But, during the month the earth's motion in its orbit changes the moon's position relative to the sun, and as consequence the time is increased to a little over 29½ days (=29^d.53059) the synodic period. Twelve of these periods or 29.53059×12=354.3671 days. The length of the year is 365.2422 days; and the difference is 10.875 days. From this it appears that if an eclipse, which occurs on a certain day in the year, is repeated the following year, the date on the average would be nearly eleven days earlier. Its occurrence would of course depend upon the position of the moon relative to the node.

Referring to Plate 3, the line of nodes N N^1 makes a little over one rotation in 19 years. The actual time is 18.6 years; *i. e.*, during this period the line of nodes labelled June 8th, 1899, will complete its rotation in January, 1918, when it will be parallel to its position in June, 1899. The angle a between the lines of nodes represents the excess, *i. e.*, the fraction of a rotation during the remainder of the year (=0^r.4).

The Saros. The word *saros* means repetition. It is a period of 6585⅓ days, and an eclipse is repeated after this interval, which is 18 years and 10 or 11 days according as there are 5 or 4 leap years included in the period. On account of the fraction of a day, which varies a little, the succeeding solar eclipse is visible from a different part of the earth's surface, due to the additional one-third of a rotation. If the period be multiplied by three we have 6585⅓×3=19,756 days, or 54 years and a little over a

DATES OF LUNAR ECLIPSES

1864	1882	1900
No eclipses		June 12
1865	1883	1901
April 10	April 22	May 3*
Oct. 4	Oct. 15	Oct. 27
1866	1884	1902
Mar. 30	April 9	April 22
Sept. 24	Oct. 4	Oct. 16
1867	1885	1903
Mar. 19	Mar. 30	April 11
Sept. 13	Sept. 23	Oct. 6
1868	1886	1904
	No eclipses	
1869	1887	1905
Jan. 27	Feb. 7	Feb. 19
July 23	Aug. 3	Aug. 14
1870	1888	1906
Jan. 17	Jan. 28	Feb. 8
July 12	July 22	Aug. 4
1871	1889	1907
Jan. 6	Jan. 16	Jan. 29
July 2	July 12	July 24
1872	1890	1908
May 22	June 2*	
Nov. 14	Nov. 26	Dec. 7*
1873	1891	1909
May 11	May 23	June 3
Nov. 4	Nov. 15	Nov. 26
1874	1892	1910
May 1	May 11	May 23
Oct. 24	Nov. 4	Nov. 16
1875	1893	1911
	No eclipses	
1876	1894	1912
Mar. 9	Mar. 21	April 1
Sept. 3	Sept. 14	Sept. 25
1877	1895	1913
Feb. 27	Mar 10	Mar. 21
Aug. 23	Sept. 3	Sept. 15

*Penumbral Eclipse.

1878	1896	1914
Feb. 16	Feb. 28	Mar. 11
Aug. 12	Aug. 22	Sept. 4
1879	1897	1915
Dec. 28	No eclipses	
1880	1898	1916
	Jan. 7	Jan. 19
June 22	July 3	July 14
Dec. 16	Dec. 27	
1881	1899	1917
		Jan. 7
June 11	June 23	July 4
Dec. 5	Dec. 18	Dec. 27
		1918
		June 23

DATES OF SOLAR ECLIPSES

1864	1882	1900
May 5	May 16	May 28
Oct. 30	Nov. 10	Nov. 21
1865	1883	1901
April 25	May 6	May 17
Oct. 19	Oct. 30	Nov. 10
1866	1884	1902
Mar. 16	Mar. 26	April 8
April 14	April 25	May 7
Oct. 8	Oct. 18	Oct. 30
1867	1885	1903
Mar. 5	Mar. 16	Mar. 28
Aug. 29	Sept. 8	Sept. 20
1868	1886	1904
Feb. 23	Mar. 5	Mar. 16
Aug. 17	Aug. 29	Sept. 9
1869	1887	1905
Feb. 11	Feb. 22	Mar. 5
Aug. 7	Aug. 18	Aug. 30
1870	1888	1906
Jan. 31	Feb. 11	Feb. 22
June 28	July 8	July 21
July 27	Aug. 7	Aug. 19
Dec. 22		

1871	1889	1907
	Jan. 1	Jan. 13
Jun 17	June 27	July 10
Dec. 11	Dec. 22	
1872	1890	1908
		Jan. 3
June 5	June 16	June 28
Nov. 30	Dec. 11	Dec. 22
1873	1891	1909
May 25	June 6	June 17
Nov. 19	Dec. 1	Dec. 12
1874	1892	1910
April 16	April 26	May 8
Oct. 9	Oct. 20	Nov. 1
1875	1893	1911
April 5	April 16	April 28
Sept. 29	Oct 9	Oct. 21
1876	1894	1912
Mar. 25	April 5	April 17
Sept. 17	Sept. 28	Oct. 10
1877	1895	1913
Mar. 14	Mar. 25	April 6
Aug. 8	Aug. 20	Aug. 31
Sept. 7	Sept. 18	Sept. 29
1878	1896	1914
Feb. 1	Feb. 13	Feb. 24
July 29	Aug. 8	Aug. 20
1879	1897	1915
Jan. 21	Feb. 1	Feb. 13
July 18	July 29	Aug. 10
1880	1898	1916
Jan. 11	Jan. 21	Feb. 3
July 7	July 18	July 29
Dec. 1	Dec. 12	Dec. 24
Dec. 31		
1881	1899	1917
	Jan. 11	Jan. 22
May 27	June 7	June 19
		July 18
Nov. 21	Dec. 2	Dec. 13
		1918
		June 8
		Dec. 3

month. At the expiration of this period the earth has made a whole number of rotations, and approximately the same part of its surface is turned towards the sun, when an eclipse may be looked for in this region. But it occurs a month later when the earth's position relative to the sun is changed. The path of the eclipse of May 5, 1864, and that of June 8th, 1918, are shown in Plate 9, Fig. 1. At the former date the eclipse was central, *i. e.*, partly annular and partly total. Referring to Plate 3, the positions of the earth and moon, labelled E and M, are shown for the dates of total eclipses on May $5^d.5$, 1864; May $16^d.8$, 1882; May $28^d.1$, 1900; and June $8^d.4$, 1918. Taking account of the leap years which are included between these dates, it will be seen that the whole number of days is constant. There is, however, a very small variation in the additional fraction of a day.

The tables give the dates of all the solar and lunar eclipses since 1864. They are arranged so that the reader will easily see in the vertical columns the advanced date of an eclipse, whenever it is repeated the following year; and in the horizontal columns or rows are shown the repetition of eclipses at intervals of 18 years and 10 or 11 days. Sometimes there is an apparent discrepancy, due to the omission in the table of the fraction of a day. For example an eclipse of the sun occurred May 16th, 1882; and was repeated on May 28th, 1900. The actual dates were May 16d.8 and May 28d.1—making the interval 18 years and 11⅓ days. In this period there were included only 4 leap years for the century year (1900) was not a leap year.

Inferences Concerning Auroras*

And Certain Physical Facts Regarding Them

By Elihu Thomson[1]

It seems doubtful if any extensive auroral display has occurred without coincident existence of exceptional areas of disturbance on the Sun. We may assume that at such times great jets or streams of electrified matter (electrons perhaps), akin to cathode rays in a vacuum, are projected with high velocities outward, and that occasionally some of these jets cross the Earth's orbit or pass near to it, being when leaving the Sun in a general radial direction, bent backwards for obvious reasons.

That electrified matter in a vacuum does move in jets or streams for indefinite distances is a fundamental fact. Moving charges of the same name in paths, straight or curved, act like parallel currents and attract one another, the more as their velocity is greater, until the static repulsion of such like charges sets a limit to further approach. It is possible that the space around the Sun may be crossed by many such jets or streams of electrified matter moving at very high velocities into the vacuous space. The coronal streamers may be the visible composite effects of the projected jets. Such electrified jets may act inductively by proximity to the earth or directly by conduction of electricity to the Earth's outer atmosphere.

In the present paper, however, it is hoped to prepare the way for further study by pointing out certain physical facts regarding the relations of auroral phenomena to the Earth and its atmosphere: to locate and give direction to the streamers seen in auroras; and to explain the nature of the so-called auroral arch, the zenith crown, and other characteristics. It is believed that the following propositions may be shown to be true:

1.—Streamers seen in auroras, singly or in composite masses, are in reality vertical, or approximately so, to the Earth's surface, nearly parallel when adjacent, and only slightly divergent even when miles apart; the divergence being due to curvature of the Earth's surface.

2.—In any aurora, the streamers appear to be located in bands or zones more or less wide in latitude extending generally in east and west direction, or forming belts or zones between parallels of latitude in which the streamers extend vertically upward like trees in a forest.

3.—In some rare auroras the vertical streamers are closely limited to a narrow belt of latitude; sometimes only 2 or 3 degrees, or even less, in width north and south, while the east and west extent of the narrow belt may be very great.

4.—In widespread auroras the belt of vertical streamers may cover great extents of latitude and extend east and west unlimited distances. This appears to have been the case in the recent great aurora of August 26, 1916.

5.—The curvature of the so-called auroral arch is an optical effect of perspective, slightly added to by the curvature of the Earth. The appearance of folded curtains of streamers is a similar effect of superposition and perspective when the active band or zone covers many degrees in latitude. It is probable that the lower ends of auroral streamers have about the same height in the Earth's atmosphere; a layer from which they stream upward to heights which vary in different displays or even in the same display. This layer probably exists at a height of about fifty miles and conducts laterally or horizontally, thus distributing the electricity discharged from it into the streamers.

6.—The convergence of long streamers toward the zenith seen in the greater auroras is purely an optical effect of perspective, the streamers being vertical.

7.—The so-called zenith crown is in reality due to bundles of streamers, nearly vertical but seen on end. They appear as patches of shifting light in or near the zenith, sometimes surrounded by apparently converging streamers from the north, east, and west, and even from the south; converging in appearance only.

8.—The convergence of streamers is of the same nature as the convergence of straight parallel railway tracks in the distance, or better, the apparent convergence toward the Sun of the sunbeams seen in dust-laden air, when the Sun itself is obscured by a small irregular cloud, or is back of a broken mass of clouds.

In April, 1883, at near the middle of sunspot period and coincidently with the occurrence of an enormous sunspot, there was perhaps the greatest auroral display seen in our latitudes for more than half a century. Telegraph lines from east to west could not be operated owing to arcing at the keys. The display as witnessed by me was characterized by colored streamers passing upward from all around toward the zenith from north, east, west and south. The coloring gave me the first clue to the true relations existing. In the north the sharply defined streamers beginning their appearance low in altitude with the usual greenish auroral light, shot, or spread upward with changes of tint, finishing at their upper ends in a deep crimson. The light patches of the zenith crown went through the *same succession* of coloring constantly, each time ending in deep crimson. This fact suggested in the strongest way that in the crown one was observing bundles of streamers on end, but of the same character as all the other streamers in the field of view. Great masses or broad bands to the east and west though steady were colored likewise. These dense masses in the east and west were unquestionably due to the composite effect of great numbers of distances to east and west; the colors less pure, blurred by distance and failure of exact superposition. Other observers at places far to east and west of my position might observe zenith crowns but composed of other streamers seen on end. A necessary consequence of the display presenting the same appearance to observers in many places far apart, up to hundreds of miles east and west, and through a zone north and south, is that the streamers everywhere were in reality vertical, or approximately so, to the earth's surface. In Chicago, a distance west of about 800 miles, this aurora was described in the same way, the appearances coinciding with my observations. The effects being so similar in places far apart, the only inference possible is the one stated, namely, nearly vertical streamers wherever seen.

In this view observations upon subsequent auroras could be made more intelligently than before. But auroral displays of great magnitude are rare in our latitudes, especially such as are so far south as to cause a belt of streamers directly over the place of observation giving the zenith crown. While several highly instructive displays occurred meanwhile, one on September 29, 1908, gave streamers converging from all directions, joining the crown in the zenith, which had the usual characteristics. My note of it says, "The rapid procession of streamers and the waving curtains make this display quite exceptional; though far inferior to that of April, 1883." On August 26, of this year (1916), there occurred a display second only in my experience to that of April, 1883, but without the remarkable coloring of the display of that year. It was observed by me in the Adirondacks, and served to confirm the inferences drawn from the earlier displays.

But on August 28 there occurred a simple type of aurora which I had only seen on two former occasions. It may be said to contain a single element only. It consisted of a single band, a belt of light, estimated at from 1½ degrees to 2 degrees wide, extending across the sky due east and west, from horizon to horizon and passing through the zenith. It lasted for an hour or a little more. At the zenith and for 2 or 3 degrees east and west thereof the band was broken into patches which shifted, faded and were replaced by others of different shapes and positions. A friend observed the same appearance in Maine hundreds of miles to the east on the same night.

The explanation of this simple type of aurora is not difficult when made in accordance with the ideas here presented. No streamers were seen as such. At the zenith they were directly over the observer, and though present were seen only on end as small patches of light constantly changing: while in the extensions or narrow bands to east and west from the zenith the luminous streak was a composite of superposed streamers in the same latitude as the observer, which streamers overlapped each other all the way down to the apparent horizon along the overhead arch east and west. They were like the palings of an extended fence located far above the observer. He would see the separate palings directly above him but only on end. Otherwise they overlap and obscure each other or one another. To appreciate fully the reality, the observer is better placed if the auroral arch when, as in this case, very narrow is not directly overhead. The usual auroral arch is the location of the lower ends of the vertical streamers extending upward from it. This location would appear from the work of Störmer and others to be in a layer of atmosphere about fifty miles high. This layer, on which the feet of the streamers may be said to rest, is probably a conducting layer like the partial vacuum in a Geissler tube.

The so-called auroral arch appears as an arch or curve merely because of perspective vision, just as a searchlight beam or a long straight cloud appears bent when crossing the sky, or as a cloud layer, known to be horizontal, appears above us as a great inverted bowl.

The streamers in the aurora of August 26 were, as in the aurora of 1883, directed toward the zenith crown from all sides. The appearance of the whole aurora when at its height, and looking upward toward the zenith, was as if one were looking into a truncated hollow cone from below its base, with the slant sides composed of ribs of light; these ribs as well as the blunt apex constantly changing, shifting, fading, and returning and traversed by waves of light of varying intensity. These appearances were not, of course, peculiar to the particular post of observation but were observed from points far east and west without substantial differences. A vivid description by Dr. C. C. Nutting is found in *Science* for October 6, 1916, pages 496 and 497. See also *Science* of November 10 to November 17, and December 8, 1916, for other letters concerning this great aurora.

The belt covered by this display was evidently of very great extent east and west and spread far to the north in latitude. One is compelled to recognize that observers far apart seeing the same appearance are looking up between nearly parallel and vertical streamers, seeing those on end as a zenith crown when directly above them; while laterally they are superposed by being back of one another at varying distances. The same auroral appearances are possible to be seen alike at different places simultaneously *only* when a system of *vertical streamers exists.*

Let us for illustration assume an extended horizontal flat surface and that there be erected above it a set of vertical and very long rods in a vertical plane extending east and west, the rods being spaced apart like the paling in a long straight picket fence. An observer on the plane faces north looking toward the rods or paling the lower ends of which are high above his position, appearing say at an altitude of 60 degrees in the north direction. The lower ends will now appear to lie east and west in an arch of curve convex upward owing to diminished angle of vision with distance, and the rods or palings will appear to converge upward if

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long enough almost to the observer's zenith, much foreshortened; the whole effect being that of perspective. Removing the point of observation further to the south, the middle point of the arch drops more and more, the arch becomes flatter, and the vertical rods appear less foreshortened and longer, while still converging toward the observer's zenith. These varied appearances, modified to a minor degree by the Earth's curvature, are just what are seen in auroras. If the vertical rods are spaced apart irregularly, increased in number and spread into a band so that they do not lie exactly in a vertical plane east and west, but in an arrangement like a long strip of forest extending east and west and of considerable width north and south (an arrangement corresponding to a belt or zone of streamers instead of a single line), the analogy to the auroral arrangement at any instant is much closer.

It can readily be seen that the recognition of the vertical relation of streamers to the Earth's surface and the nearly constant level of their lower ends simplifies to a great extent the study of auroras, particularly the determination of the total height reached by them, curvature of the Earth being allowed for. The effect of this curvature will be less the higher in altitude the auroral arch, or the nearer it is to being overhead. The streamers are often observed to rise from the arch first as short streamers, gradually developing and extending upward until their upper ends are a few degrees from the zenith. When they originate in an arch which is of low altitude and extend nearly to the zenith, as they appear to do in the greater displays, their length must be hundreds of miles, possibly in some instances reaching one thousand or more. It would appear that no limit can be set for their possible height. In most auroras, however, the visible extent of the streamers is more limited. A low altitude auroral arch implies great distance north from the observer and for a given actual streamer length a less apparent height or length. Paulsen's class of auroras without streamers may mean either that the streamers are too short and too many to be noted separately, or that the electrification is too feeble for their development, the observed luminous glow being due to flow of current in the conducting layer itself, an arch forming horizontally, but without outward projection.

If our assumptions are approximately correct the arch of an aurora, if located farther north than about 600 miles from an observer, will be below his horizon, but the streamers extending upward from it if long enough may be seen. Auroras far north of this will probably be invisible or be seen merely as a luminous glow well down on the northern horizon. When the breadth in latitude of the auroral zone is great and the display is seen from the south, the streamers may overlap or be arranged in apparent curtains or folds, the lower ends of the streamers being in that case at varying apparent altitudes above the northern horizon even when in the same general line of view. In such case they may be superposed in the line of sight and therefore be composite, or increased in apparent length owing to imperfect superposition in their lengths with respect to those back or front of them. It is believed that these and like considerations will suffice for the explanation of observed appearances of auroras in spite of their great variety.

The direction of streamers, as indicated, being vertical to the Earth's surface, is suggestive of electrical discharges, ions or electrons projected outward into space from the conducting layer of our atmosphere; discharge into space in which the mean free path is unlimited. These discharges might have their origin in a charged conducting layer in which the potential suddenly rose to a critical value, or might be brought about inductively by the presence in the space around the Earth of opposite charges possibly arriving in great jets from the Sun. Further the charges might be communicated to our air from such jets followed by discharge into space beyond.

A sudden uplift of a highly charged thin air layer itself in waves or ridges might easily disturb the normal potential distribution and precipitate discharges from the crests. Atmospheric currents or displacements may cause such uplifts as when a colder stream strikes down under a warmer charged layer.

A set of vertical streamers would deflect a compass needle on the Earth's surface one way or the other depending on whether such streamers exist to the north or to the south of the position of the compass. Observations seem to confirm this but more work is needed The direction of the compass deflection would determine the direction of the virtual streamer currents to which the deflection was due. Aside from these and other considerations, the effort has, however, been to present in this communication a rational theory which will at least enable a proper conception of the actual space relations of the visible portions of an aurora in relation to the Earth's surface to be obtained; and to place on record ideas which through many years of consideration by the author have seemingly received at each appearance of an aurora with streamers, repeated confirmation.

While I have in former papers, as in "Thoughts on Osmical Electricity," an address before the Franklin Institute of Pennsylvania (December 19, 1893), and notably in an address on "Atmospheric Electricity" delivered at Princeton University October 21, 1909, and published in *Science*, December 17, 1909, pages 857 to 869, given a brief sketch of some of the views presented, particularly the outward direction of the streamers, later observations have served to provide cumulative evidence and extend their application.

The Lumen Unit of Illuminating Power

THE term "lumen," proposed by A. Blondel in the past century, has become familiar to scientists, but has not gained much favor with the public. People understand what light a 16-candle lamp will give, but the lumen does not convey anything to them, and when they hear that the definition of the lumen involves the term spherical candle-power, they naturally object to being bothered with a new name for something which they had learnt to understand in another sense. The answer of the scientist is that candle-power is an inadequate and indefinite term, because there are several candle-powers, that spherical candle-power does not mean what it is frequently believed to signify, and that under such conditions it is best to introduce an altogether new, preferably foreign, name to put an end to misunderstanding. To speak of lumen or of flux of light conforms, moreover, to modern conceptions about radiations, and popular expressions such as a lamp "shedding light" and "floods of light" indicate that the common conception of light is indeed that of a flux and not of an illumination. This was pointed out by Mr. C. C. Paterson in March, 1915, when the Illuminating Engineering Society discussed the "lumen" for the second time, the first having been in May, 1914.

The question came up for the third time in the meeting of the same society held on January 16th, and the general adoption of the lumen is no longer doubtful. On the second occasion Mr. A. P. Trotter had pleaded for the retention of the candle-power, mainly, probably —we do not remember his words—because it meant something to those directly interested, the lamp maker and buyer. Meanwhile, however, illumination as a science has been making strides, the leading electric companies and others offer lumens instead of candles, and the public must in such matters be guided by the scientific expert. The chief papers presented in the last meeting were by Prof. J. T. Morris, of the East London College, and by Mr. F. W. Willcox, of the British Thomson-Houston Company. The latter had taken pains, by preparing many diagrams, to explain the inadequacy of the candle-power and the advantages of the lumen. To speak of candle-power is like speakng of the pressure of a pump; what one wants to know is the output of light and of water. The amount of water that a lake can yield might be measured by drawing line across the lake and measuring the depth along that line—provided the basin were regular, and all the lakes in question were of the same shape. If a ditch were excavated along that line the depth would be much increased, but the capacity of the lake very little. Similarly the candle-power of a lamp can be increased by turning part of the globe into a reflector, and the candle-power depends much on the number, distribution, thickness and shape of the filaments. As that is fully recognized, of course, the mean spherical candle-power—M.S.C.P.—is stated instead of horizontal candle-power or the power at a certain angle. But the M.S.C.P. is difficult to determine and to interpret, just as difficult as the lumen, and one might, therefore, go one step further and adopt the lumen.

The lumen is practically defined as the flux of light received by a surface of 1 square foot, when under an illumination of 1 foot-candle over its area. The flux of light emitted by a source is 4π times its M.S.C.P.; 4π is equal to 12.57. Thus a 50-watt tungsten lamp operating at 0.8 candle per watt. (or 1.25 watts per candle-power) yields approximately 500 lumens. The lumen, therefore, is, in a sense, nothing but a multiple (12.57 times) of the mean spherical candle-power. But we can speak of light flux or lumens per zone of 10 degrees, while it would be very cumbersome to express our meanings in M.S.C.P. A source of light of 1 candle suspended in a sphere of 1 foot radius would shed one lumen over each square foot of that surface, and as the total surface is $4\pi \times 1^2 = 12.57$ square feet, the total light flux will be 12.57 lumens. When the source is not a point the lumens have to be determined in different zones; the sum of these determinations gives the total lumens emitted by the lamp. When the determinations are subsequently repeated for the lamp as fitted, in a particular case, with globe, reflector, etc., we find the lumens available upon a certain area as per cent of the total; this percentage is the "utilization efficiency," which will also depend upon the color of the ceiling and walls and other circumstances. When a room is to be lighted, the area in square feet A is multiplied by the desired intensity in foot-candles C; the product gives the net lumens required. To arrive at the total lumens L, that figure has to be divided by the utilization efficiency E. Knowing the total lumens, the illumination engineer consults the lumen tables of various sizes of lamps; the number of lamps required is then the quotient AC/EL.

There was no real opposition to the adoption of the lumen. Mr. A. P. Trotter rightly emphasized the need of a proper definition of the lumen, and the Illuminating Engineering Society resolved, on the suggestion of its honorary secretary, Mr. L. Gaster, to issue a pamphlet explaining the use and value of lumen rating. In the definition of the lumen we quoted above, after Professor Morris, use was made of the foot and the foot-candle. But the lumen is really independent of the system of measures, and would be same if feet were replaced by meters; the factor falls out in the calculations. The unit of intensity of light, the international candle, is, and has been, for some years the same for Great Britain, France and the United States. Thus the objection is met that the present adoption of the lumen would probably necessitate a further, though slight, change when the British system of measurements will abdicate in favor of the metric system, a change that the war is hastening on. Until a few years ago the British candle was not quite the same as the United States candle; it was also a little smaller than the Hefner candle, very slightly larger than the unit of the Laboratoire Central d'Electricité at Paris, and only a tenth, roughly, of the Bec Carcel. But those differences disappeared when the three States mentioned adopted the "international candle," and it seems very likely that the international candle will really become international, and that the continental nations will adopt the lumen rating. Many American lamp makers have done this, and Professor Blondel informed the meeting in writing that the Société de Physique de France and the Société de Electriciens favored this adoption.—*Engineering.*

Utilization of the Tar from Gas-Producers Using Lignite as Fuel

THE tar is difficult to treat by the usual distillation processes owing to its high water content. By mixing 1 part of the tar with 1½ parts of powdered lignite, a stiff pasty mass is obtained which can be formed into rods. The rods are cut in pieces and then distilled, yielding 36 per cent of crude oil and a dark grey, crumbly residue having a heating value of about 7,000 calories. The tar may also be treated by extraction with petroleum spirit, either directly or after making into briquettes with lignite powder as described above. In this way about 40 per cent of a brown grease can be isolated which can be used as a substitute for animal oils in the leather industry. From the residue about 20 per cent of resinous matter can be extracted with benzene, The grease extracted by petroleum spirit can be decolorized and decolorized by treatment with ozone in the presence of sodium carbonate solution.—F. FISCHER and W. SCHNEIDER in *Stahl U. Eisen.* From a note in the *Journal of the Society of Chemical Industry.*

Perfume Industry in Southern France

THE region lying on the south coast of France in the neighborhood of Nice was always noted for the production of perfumes or flower essences, and it is reported that the war conditions have led to great efforts to increase the production of this class of material. In view of an extensive exportation to the rest of Europe as well as to America after the war, there is at present a most unusual activity in the distillation of jasmine and lavender flowers of the recent crop in Nice, Menton, Golfe Juan and other centers in the Maritime Alps region. Other products which have already afforded a great yield this year are orange flower, rose, geranium, mint, etc. In fact this region is making great preparations to complete after the war with the synthetic products coming from Germany which were so extensively imported, and the natural products will now take a prominent place.

Examining leaf tobacco in the appraisers stores

Appraising miscellaneous merchandise in New York

How Uncle Sam Protects His Revenues

The Work of the Appraisers Stores in New York

By A. H. Pearson

PERSONAL liberty in the United States is so complete that we scarcely realize there is a powerful central government to watch over our destinies and make us comport ourselves with proper regard for the rights of citizens in our neighboring States. Frequently, an American's first real contact with Federal power comes on his return from a trip abroad, when he is advised that he must pay duty on goods that he has brought with him. He may have looked with contempt on the poor foreigner who must submit to the pettifoggery of an officious government, and he may be returning with a smug "better-than-thou" attitude, only to receive a rude shock to his complacency as the customs officials board the vessel and make him swear out a statement of his dutiable personal effects. Then, no metter if he does consider it an invasion of his rights as a freeborn American citizen, he must submit to having his trunk opened, and searched more or less perfunctorily, to make sure that he has not perjured himself. He may even be called aside to answer searching questions about a certain piece of jewelry. Now, how did Uncle Sam know that he had that trinket? For the first time he is aware of a spy system, not unlike that of Russia, which reaches out beyond our shores to foreign lands and keeps track of the purchases of the American tourists. Despite the humiliation of being treated as a smuggler, he cannot help but feel a great respect for the omniscience of a government whose existence he barely realized up to that moment.

Although examination of traveler's baggage is the most troublesome work that the Custom House has to deal with, it is a paltry business compared with the collection of duties on general merchandise. Despite the far greater attention to personal baggage, smuggling still continues among tourists, especially those of the gentler sex, who display remarkable ingenuity in concealing their dutiable goods. One customs official hopelessly admitted that "women are born smugglers, and we cannot hope ever to suppress them."

As for general merchandise, the opportunities for smuggling are so remote, the coöperation between the Government and the importers themselves is so complete, and the penalty for smuggling is so severe as compared with the reward it offers, that practically no goods enter the country without paying duty. Take diamonds, for instance, which one would suppose could very readily be introduced into the country because their value per size is so enormous. Not only does the Government keep track of purchasers of diamonds abroad, but the dealers do as well, and they are constantly on the lookout for smuggled stones, realizing that it is to their own interest to report any stones introduced without paying the required tariff. Furthermore, to make it unprofitable to smuggle the stones into the country, the tariff on them was reduced several years ago from 25 per cent to 10 per cent.

Some idea of the enormous amount of work involved in keeping track of the goods that enter this country may be obtained by a visit to the Appraisers' Stores on the lower west side of New York. The building is ten stories high and takes up an entire block, while across the street is an annex of no mean size. In these buildings at least 10 per cent of everything that comes into New York from foreign ports must be examined. A sample of literally everything under the sun finds its way at one time or another into the Stores, and no matter what its character may be, whether a fifty-karat diamond or a penny doll, it must be gravely considered and its value accurately and scientifically determined, so that the proper custom duty may be levied thereon. To handle this enormous quantity of material engages the attention of 938 men, of whom 134 are examiners. The duties of the examiners are exceedingly difficult. Each man has a certain classification assigned to him, and he must be prepared to determine the wholesale value of any of the various articles that might turn up under that classification. He must be able to tell of just what material or materials the article was made, how much the materials were worth in the market from which they came, and just what was the value of the labor which was expended upon it. Not only that, but he must know the market values of the materials and labor at the time of shipment. This must be determined on his own knowledge

Tea testers at work

and not on the word of the shipper. He cannot depend on anyone else, but must stand on his own statement, which he must be ready to back up with incontestable, evidence in case the importer carries an appeal to a higher court. He must be able to detect all the tricks with which unscrupulous manufacturers delude the ignorant public. For instance, in the textiles department, the examiner must be able to tell whether a piece of goods contains cotton, linen or silk, and in what proportion. Having determined this, he must know the quality of the material used in making it up. If it is of silk, he must determine whether the silk is artificial or natural. If natural, what kind of silk, and where it came from. If he is in doubt about the matter, he refers a sample to the laboratory, where the fabric is subjected to a chemical test in order to determine accurately what its composition may be. Naturally, an examiner acquires before long such an experience as to qualify him as an expert, an experience that it is impossible to obtain anywhere else.

Recently, curiosities, works of art, and antiques, over a hundred years old, have been admitted free of duty.

The examiner who has to appraise the work of artists has an exceedingly difficult task. In many cases it is not at all easy to distinguish between spurious and genuine old masters. The work of these examiners is of undeniable value to the country in preventing the importation of counterfeits.

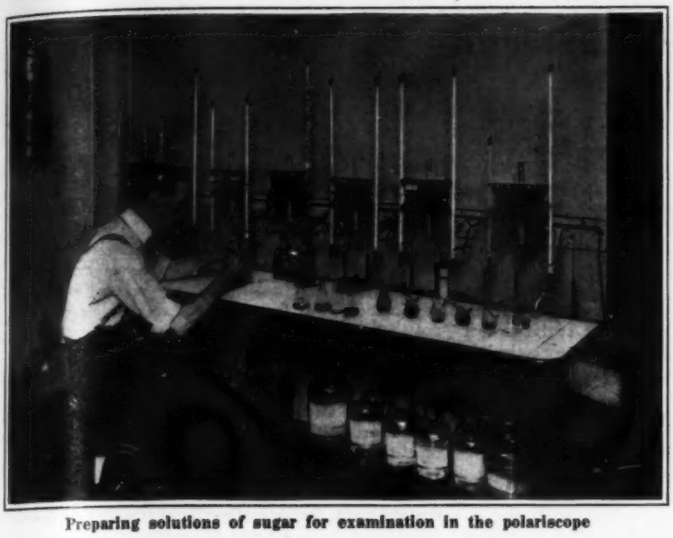
Similar protection against fraud is found in the case of tea. No duty is levied on tea, but all tea must be examined for purity before being admitted into the country. In the tea room of the New York Appraisers' Stores a hundred thousand samples of tea must be tested per year. One of the photographs shows the manner of testing. Each cup contains a different sample of tea identified by a number marked on the bottom of the cup, and one of the cups contains a standard sample. Which one it is the examiner does not know, for the identification of this sample also is marked on the bottom of the cup. The examiner then proceeds to arrange the cups according to the color and taste of the tea. After the grading is done the samples are thrown away and the cups turned upside down to show the identifying numbers. All the samples on one side of the standard are passed as good tea, while those on the other side are rejected. To make sure that no error has been made the test is repeated with a second set of samples. In order to detect any pigment used in the tea the leaves are mashed on a piece of white paper, and then the paper is examined with a microscope for faint spots of coloring matter. The tests are very rigid and thorough, and the United States may pride itself on having nothing but pure tea to drink.

Perhaps the most tedious work at the Stores is the testing of sugar. The tariff on sugar depends on the proportion of cane sugar the samples contain. This is determined accurately by means of a polariscope, which analyzes the light that passes through samples of the sugar syrup. In the case of sugar only samples are brought to the Stores, and as a check upon the examiner, two samples out of each barrel are given him. Each sample bears its own number, but the examiners have no means of determining which two came out of the same barrel. Nevertheless, his work must be so accurate that when like samples are paired again the readings will be practically identical.

The laboratories of the Stores are also kept busy with quantitative analyses of various chemical products, particularly in the search for alcohols in medicines, etc. There is also a section devoted to metallurgical analyses.

A department of vital importance to every citizen is that which rigidly investigates all drugs and medicines brought into the country. These are not only examined chemically, but are closely scrutinized by expert botanists to insure against adulteration and substitution.

Obviously it would be impossible to examine every article imported into the country, and so it is the practice to bring at least ten per cent of a shipment to the Stores. If the shipment consists of but one or two cases of goods at least one case must be examined. The cases that go to the Stores are picked out at random by the examiner. He compares the contents of the case with the invoice and then investigates one of the articles under the invoice minutely in order to determine its quality. If this tallies with the specifications the case is passed. In the case of leaf tobacco, every package must be opened, in order to determine whether the leaves are good enough to be used for wrappers, which must carry a duty of one dollar and eighty-five cents per pound, or whether they are fit only for fillers, which pay thirty-five cents duty.



Preparing solutions of sugar for examination in the polariscope

Volumetric table where acids, alkalies, drugs etc., are analyzed

Petroleum in England*

THE announcement of a paper on "The Occurrence of Petroleum in England" may remind the reader of the traditional chapter on "Snakes in Ireland" in some natural history of Ireland. "There are no snakes in Ireland," that chapter is said to read. "There is no petroleum in England," people will say, and, broadly speaking, they are right so far. Yet there is sufficient justification for bringing the "Occurrence of Petroleum in England" before the Institution of Petroleum Technologists. This was done recently by Mr. William Forbes-Leslie, M.B., F.G.S., of Eastbourne. Mr. Forbes-Leslie was not in a position to announce—even with the reserve which the present political aspect might exact—the opening of powerful petroleum wells in this country. But dealing scientifically, for the first time probably, with the possibility of economic petroleum deposits in this country, he could marshal up strong evidence for the preservation of free oil in large quantities in certain localities, and for the "suspicious" juxtaposition of those deposits to tectonic underground structures capable of detaining such free oil. His evidence, it will be understood, is essentially historical and geological. Finds of free petroleum have been reported from time to time from many parts of the country; but they all had their day, the flows did not last long. The oil-shales of commercial value that are being worked are mostly situated in the north of Britain; Mr. Forbes-Leslie finds in the south and east indications in vast quantities of oil-shales overshadowing those being exploited. The oil, he suggests, is probably in the Carboniferous, not in the Jurassic, and at considerable, though not by any means prohibitive depths. Wherever the folding in the Carboniferous led to the development of good anticlines, and where the suitable materials were present, he argued, there oil might be expected—he spoke cautiously of a suspicion—and the frequently observed escapes of petroleum certainly support him. Yet his paper was in a sense revolutionary, as Mr. Cunningham-Craig put it; and it cannot be said that the many pages of tectonic geology through which Mr. Leslie hurriedly took his audience, without the aid of a map, though the explanations presumed a very intimate knowledge of geographical detail, were very instructive to his listeners. The map is to be added, and the paper can then be studied with the care it seems to deserve.

Discoursing on Geological Mapping four weeks ago, before the same Institute, Mr. Lister James had insisted that a proper geological survey should be made before erecting any derricks, and Dr. Strahan, Director of the Geological Survey, last winter, in the Royal Institution regretted that much valuable geological evidence which might be collected, and was collected, during commercial prospecting for minerals was entirely lost to science and to the country. With more abundant and reliable data at his disposal, Mr. Leslie's task might have been easier. He pointed out that petroleum was first struck in the Eastern United States within the limits of the Silurian and Lower Devonian. The Tertiary of our country did not appear to contain liquid oil. The bituminous shales, from which oil could be obtained by artificial means, were in the lower divisions of the Upper Oolites, and, with a possible exception of Liassic oil near Bristol, no real oil zone seemed to have been reached before the Carboniferous was encountered. The actual oil escapes in Norfolk (on the line from King's Lynn to Ely) and elsewhere were on the surface of Jurassic rock, but probably of Carboniferous or earlier origin.

*Engineering.

To investigate this problem the position of Britain in the geological record had to be examined in connection with the geology of the Continent, of which the British Isles once formed part, and from which they were separated by the Channel and the North Sea, the latter nowhere more than 95 feet in depth. Great Britain stood on a broad Continental border, over which the depth of the sea did not exceed 600 feet.

Having followed, as we have mentioned already, the axial lines, folds, anticlines, synclines and faults, which the action of tectonic uplifting and depressing forces, thrust and other factors had produced in England and Wales, in detail over the country, especially over the eastern Midlands, Mr. Forbes-Leslie traced the recorded occurrences of seepages (oil leakages) to tectonic features, to the meeting of anticlines, and to faults. Those occurrences were more frequent in the middle and eastern zones than in the northern zones. The founder of the Scottish shale industry, Dr. James Young, seventy year ago, manufactured paraffin wax from oil flowing from the

Testing sugar solutions with the polariscope

Reddings Colliery at Alfreton, in Derbyshire; for a year or more up to seventy barrels of oil had been gained weekly and been sent up to the refinery in the north which Young had started. Minor discharges were recorded from many other spots. Within quite recent years a most remarkable oil find had been made at Kelham, near Newark, where, at 2,452 feet—Mr. Leslie afterwards gave the exact depth as 2,439 feet 2½ inches—13 feet of a grey sand had been struck, below the millstone grit, from which oil had forced its way up for eleven months through the high column of water in the hole. We do not quite understand why this coming-up of an oil lighter than water should be considered remarkable. This statement was subsequently confirmed by Mr. J. Ford, who had made the boring, and who added that the boring had been stopped when they got into the limestone underneath that sand. Mr. W. H. Dalton suggested during the discussion that at Kelham the fault had itself been struck. Mr. Leslie thought that those oil sands were merely the upper sands of a productive series of unknown thickness. In any case the occurrence seemed to be one of those connected with the three anticlines of the eastern depression of England, the Ashover-Sherwood-Kelham anticline, the Castletown-Retford anticline and the Ouse valley fold. The latter, the Ouse valley fold, or King's Lynn-Downham anticline, was likewise very important; the escapes occurring there had, by others, been ascribed to other sources before Mr. Leslie had traced that fold. Those Norfolk deposits of oil-shale, containing probably 30 or 40 gallons of oil per ton, 75 per cent of this being in the form of free oil, seemed to amount to hundreds of thousands or millions of tons, and enormous bulks of petroleum were apparently absorbed in those strata. The shale-oil was, as just indicated, of a dual character, the greater part being free oil, not requiring any temperature above 350 deg. F. for its extraction. We see that Mr. Leslie ascribes with Mr. Cunningham-Craig, a common origin to shale-oil and to free oil, a question into which he did not enter, however. Nor did he, we should mention, dwell upon the intermittent character of all the British oil flows recorded.

The lecturer referred to many other oil seepages, at Brigg, in Lincs. (where the oil apparently comes from the Kimmeridge shale); at Donnington (Lincs.), in the Middleton-Flamborough Head fold; at Middlesbrough, and other places. He attached most importance to the occurrences in north-west Norfolk, where the first oil sands should be found at depths between 2,500 and 3,000 feet. There would probably be coal measures as well there. He dealt in particular with the eastern depression, as most interesting, and has not yet attempted apparently to trace the historical oil occurrences at Whitehaven, in the Wigan district, at Swinton, in Staffordshire, Shropshire and near Barnstaple in the same way. It remains to be seen whether other geologists will support Mr. Forbes-Leslie in his conclusions; Mr. Cunningham-Craig and Dr. Henderson agreed to a certain extent. All geologists will, however, welcome further drilling and further research. Deep boring will be required for this as for other reasons. Let us hope that it will be successful.

Eskimo Migrations in Greenland

At Holstenborg, Greenland, well within the Arctic Circle, is one of the northernmost outposts of settlement and here V. C. Frederiksen, a resident missionary, has published a monthly journal, a volume of church hymns, a brief history of Greenland, and several literary translations, all in the Eskimo language, while at the same time carrying on archeologic investigations and making pastoral calls by dog sledge and kayak at the small native settlements scattered along 300 miles of dangerous coast. Pastor Frederiksen, in another monthly journal called *Atuagagdliutit*, or "Reading Miscellany," published at Godthaab, Greenland, has expressed some very interesting views on Eskimo migrations according to an abstract by James Mooney in the *Journal of the Washington Academy of Science* (Vol. 6, 1916, No. 6, pp. 144-146.) The evidence of linguistics, geography, and archeology led him to conclude (1) that the Eskimo tribes reached Greenland from an original nucleus in the extreme west, (2) that they traveled southward along the coast to the east, and (3) that they decreased in number toward the north owing to the scarcity of game and building material. He believes that the Norse occupation about 1000 A. D. made a wedge between the east and west coast Eskimo and that natural communication was again established only after the extinction of the Norse colony about 1490. Some of the northerly tribes on the east coast starved to death; some of the southerly tribes were saved from a like fate at a later period only by contact with Danish colonists. The superior capacity for civilization of the South Greenland Eskimo is explained by a strain of old Norse blood.—*Geographical Review.*

Structural Engineering*

Some Things We Do Not Know

W. M. Wilson, M. W. S. E.[1]

CRITERION OF THE QUALITY OF A DESIGN

THE fact that a structure has not failed is not proof that it was properly designed. Structures are generally designed with a factor of safety of about 2, based upon the elastic limit. The fact that the structure has not failed is evidence ouly that all members have a factor of safety of at least 1, and there is no evidence to show that some members do not have an excess of material.

If a design is good because the structure does not fail, the design of the ancients should be copied, whereas they are known to be so wasteful of material as to be prohibitive.

For a design to be good it must be well balanced. Although a structure *must* be safe, it also *should* be economical. In stating that a structure must be well balanced, I do not mean that the ratio of strength to stress necessarily shall be the same for all members. Many other factors besides strength must be considered. If the failure of a small part will destroy a whole structure, the factor of safety in the small part should be greater than the factor of safety for larger or less critical parts. This is illustrated by the second Quebec bridge disaster. Because of saving in shop costs it is sometimes cheaper to build a member that is too large then it is to build a member that will just carry the load to which it is subjected. Because of the small amount of material involved, it is often cheaper to use an excess of material than it is make an exact analysis of the stresses. An increase in the live load may cause a much greater increase in the total stress in one member than in other members of the same structure, therefore, if there is a possibility of an increase in the live load, such a member should have a greater margin of safety in the original design than other members of the structure. Some members upon which a structure as a whole depends, such as the end post of a truss, are subject to accidental forces, and, although neither the probability of the load occurring nor the magnitude of the stress produced, can be expressed mathematically, these accidental forces should be considered in designing the member. In some structures metal is added above what is required to carry the stress, because the structure is especially liable to corrosion. In such a case all members are not increased by the same relative amount. In some structures ridigity and "feel" of the structure must be considered. This is especially true of railroad bridges, in which it is not only necessary that the bridge figure safe but also it must ride as it is safe. Bearing in mind the factors that have just been enumerated, design, to be good, must be well balanced. Although it is good engineering to make some members of a structure larger than stress alone requires when there is a definite object to be attained, it is not good engineering to make a member unnecessarily large because of ignoranc of its true function.

DESIGNING NOT SOLELY A MATHEMATICAL PROCESS

It is not true, as the beginner is likely to believe, that the design of a structure is made up only of exact mathematical processes, and that the results are therefore infallible. In general with known forces acting upon a structure the stresses are determined mathematically, and with the total stress in a member known, the area of the section is determined by dividing the stress by the allowable unit stress for the material, after which it remains to select a section having the required area. Thus the structure is designed and apparently only mathematical processes are used. The thing that has been entirely overlooked, however, is the fact that for every mathematical equation used, assumptions have been made in deriving the equation and these assumptions have been entirely neglected in applying the equation. The accuracy of the results depends not only upon the accuracy of the mathematical processes, but also upon the accuracy of the assumptions upon which the mathematical processes are based.

ASSUMPTIONS IN THE DESIGN OF A STEEL RAILROAD BRIDGE

To illustrate the importance of assumption, consider the design of a steel railroad bridge. The design may be divided into four parts, as follows: determination of the loads to be carried; calculations of the stresses; design of the members; and design of the connections.

The load upon a bridge consists of the dead load, live load, impact, wind load, and lateral forces due to a moving train. The dead load can be determined satisfactorily from the design of other bridges. The live load, although fixed arbitrarily, can be accepted as quite satisfactory except that future live loads are problematic. Impact is usually computed from the live load by empirical formula based more or less upon tests. Wind forces and lateral forces due to a moving train cannot be determined satisfactorily. The lateral forces to be used in a design are usually given in specifications. Specifications, however, differ, and some engineers feel that all the specifications are wrong in some particulars as regards lateral forces. Clearly the lateral force to be used in the design is not the same for bridges in all locations.

The stresses in a bridge, as usually computed, due to known forces, are based upon the following assumptions:

1. All external forces are applied at the panel points.
2. The weights of the members are applied at their ands.
3. The axial gravity lines of all members connected at a panel point are concurrent.
4. The connections are frictionless hinges.

Assumption 1 is usually fulfilled. Assumption 2 is fulfilled for all practical purposes for vertical members; the fact that it is not fulfilled for horizontal and inclined members does not produce appreciable error except in the case of long members. In the case of long members a correction can be made for the bending stress due to the weight of the member. Assumption 3 can be and shoyld be fulfilled, except as it may be desirable to have a little eccentricity in a joint to offset the error in assumption 2. Assumption 4 is not fulfilled, and because it is not fulfilled sceondary stresses are produced which may be 50 per cent or more of the primary stresses.

In designing a member it is customary to assume that the stress is uniformly distributed over the entire section. Likewise in designing a riveted connection it is assumed that the stress is uniformly distributed among all of the rivets. These two assumptions are discussed later.

ASSUMPTIONS WHICH ARE NOT CORRECT

Omitting the assumptions relative to the loads, it is apparent that the assumptions which enter the design of a bridge which demand special attention on the part of the designer refer to the following: secondary stresses; distribution of stress in a member; and distribution of stress in a cnonection.

Secondary Stresses: One of the fundamental assumptions in stress analysis is that connections are frictionless hinges. If a truss having frictionless hinges is deflected, the members meeting at a joint are free to rotate relative to each other and no bending stresses are produced in the members. If, however, the connections are rigid, when a truss is deflected the members are not free to rotate relative to each other, and bending stresses, known as secondary stresses, are produced. These secondary stresses can be determined mathematically. While all are willing to admit that, theoretically, secondary stresses exist, many, apparently because of the elaborateness of the calculations necessary for their determination, look upon them as something invented by the mathematician for the further torture of the soul of the engineer. The strain-gage, however, has come to the support of the mathematician and secondary stresses are known to be a reality.

Secondary stresses equal to 50 per cent of the primary stresses are common. To just what extent such secondary stresses increase the danger of failure, however, is not clear. Upon first thought it would seem that inducing a secondary stress equal to 50 per cent of the primary stress would increase the danger of failure as much as if the primary stress were increased 50 per cent. This, however, is not the case. A secondary stress, being a bending stress, is not uniform over the section, and if the maximum values of the combined primary and secondary unit stresses exceeds the proportional limit of the material, Hooke's law no longer applies, and there will be but little increase in the combined stress beyond the proportional limit. Just what value of combined primary and secondary stress can be used as a working stress has not been agreed upon by engineers.

Although secondary stresses can be computed, the calculations are so long that they are not at present made except in the case of the more important structures. For ordinary structures secondary stresses do not appear in the calculations and are not mentioned in the specifications. The view is held by many engineers that secondary stresses need not be considered, since with an allowable unit stress of 16,000 lbs. per sq. in. a secondary stress to 50 per cent of the primary stress produces a total stress considerably below the yield point of the material. If secondary stresses were equal in all members of a truss and in all types of trusses this position could be defended, since the method would result in a structure for which all members would have the same factor of safety. But secondary stresses do not bear the same relation to the primary stresses in all members of a truss, and the secondary stresses are not the same in all types of trusses. The result is that designs are not properly balanced. Furthermore, we are unable to judge intelligently of the relative merits of different type of trusses.

The calculation of secondary stresses is a long and tedious process, but, like a long journey, it shortens with repetition. In the early days of bridge design, moment tables were unknown. Moreover, to the student first introduced to the problem of primary stresses due to moving loads, the determination of the stresses in a six-panel truss due to a Cooper's loading is almost an overwhelming job, even with a moment table, whereas, to the experienced designer equipped with modern tables it is a small part of a forenoon's work. If we once recognize the necessity of determining secondary stresses, may we not hope for a similar development in the methods by which they are determined? The design of short and medium length spans has been standardized, and if the secondary stresses are computed in a few of each of the common types of trusses, may we not hope to establish empirical laws by means of which the secondary stresses in other trusses may be determined in a comparatively short time? Thus it will be possible to design all members with approximately the same factor of safety after secondary stresses have been included.

The resulting more perfectly balanced design would be a sufficient reason for making an extended systematic study of secondary stresses in standard trusses. There are, however, other results to be obtained from such a study which are even more important. A more intimate knowledge of secondary stresses would result in the abandonement of types of trusses having high secondary stresses and the substitution for them of trusses having low secondary stresses. An illustration of this is to be found in the attempts to use the K type of truss of the Quebec bridge for shorter spans. Moreover, being confronted with high secondary stresses, the engineer would exercise his ingenuity to devise details which would reduce the secondary stresses. This is illustrated by the recent use of pin connections between the floor beams and trusses to reduce the secondary stresses in the vertical posts of trusses.

Distribution of Stresses in a Member: The area of the section required for a member subjected to a known stress is obtained by dividing the total stress by the allowable unit stress for the material. This is virtually equivalent to assuming that the stress is uniformly distributed over the area of a section of the member.

Tests show that if an angle is riveted to a gusset plate by means of rivets in one leg only, the full strength of the angle cannot be developed. Members of trusses are much larger than the single angles tested and some portions of the section are a considerable distance from the central point in the connection. Engineers recognizing the necessity of attaching the member to the gusset plate over as large a portion of the section as possible. If portions of the main member, such as the outstanding legs of angles, are not in contact with the gusset plate these portions are connected to the gusset plate by means of lug angles. In considering the value of lug angles, it is well to bear in mind that where there are several paths for a stress the portions of the total stress which travel over the different paths depend upon the relative stiffness of the paths. To illustrate the truth of this statement consider the apparatus represented by Fig. 1. The blocks A and B are connected by two wires, one is a small tightly drawn wire and the other is a large slack wire. By inspection the stress is transmitted through the rigid path provided by the small tightly drawn wire and but little if any of the stress is transmitted through the less rigid path provided by the large slack wire. Consider again the case of an angle riveted to a gusset plate. The connection of one leg to the gusset plate by means of a rivet holding the two

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pieces in immediate contact is very rigid. A lug angle attached to the gusset plate and to the outstanding leg of the main angle, comparatively speaking, is not rigid. The result is that a strain which ruptures the leg of the angle riveted directly to the gusset plate produces but little stress in the lug angle. This statement is supported by tests. Reasoning by analogy, if the different parts of a connection are not equally rigid the stiffer parts may be overstressed, if not atcually ruptured, before the more flexible parts of the connection receive an appreciable stress. It is true that as the stress in the most rigid part passes its proportional limit a part of its load is automatically transferred to other parts of the connection, but in the case of the angle connected to the gusset plate one part of the connection is so much more rigid than the other that the two parts act in succession instead of in unison. The same action is even more likely to occur in the connections for large truss members. Let me repeat, the portion of the stress which follows one path depends not at all upon the strength of the path, but depends entirely upon its rigidity, and the stress in a member is not uniformly distributed over a section at the end unless the different parts of the connection are properly proportioned, one to another, relative to their rigidity.

It is the writer's opinion that for ordinary working loads the stress at the end of a member is not uniformly distributed over the section, and that the maximum unit stress is materially greater than the total stress divided by the area of the section. Furthermore, it is the opinion of the writer that ultimate failure of many members connected as they are in bridges, will occur at a stress materially below the ultimate unit strength of the material multiplied by the area of the section of the member. Since the advent of the strain gage the first statement can be either verified or disproved by means of field tests of bridges under normal working conditions without a prohibitive expense. The second statement can be checked by tests to destruction.

The important thing to be revealed by such tests is the efficiency of the different types of sections. This is illustrated by the recent tests made by the Bureau of Standards at Pittsburgh as reported in Engineering News July 13, 1913. For some of the columns tested failure was due to a part of the section failing independently of the other parts of the section. That is, the full section of the column was not developed. If a member having one section can develop a stress of only 80 per cent of the product, area times unit strength of material, whereas if the member had a different type of section it could, because of the better type of connection possible, develop 90 per cent of the product, area times unit strength of material, then other things being equal the latter type of member should be used. At present we have but little, if any, reliable data relative to the efficiency of connections for different types of members. We have many opinions, but few facts.

Distribution of Stresses in Connection: The discussion of the distribution of stresses in a member is equally pertinent to the distribution of stresses in a connection. If the connection is made up of a number of parts each of which is to take a certain prescribed portion of the total stress, each part must have just sufficient rigidity to enable it to take its portion of the total stress. This condition it is practically impossible to obtain.

In a riveted connection the stress is not uniformly distributed among the rivets. The rivets are distributed over a considerable distance and the intensity of the stress in the gusset plate at its outer edge is zero, whereas the intensity of the stress in the member at the same point is a maximum. The intensity of the stress in the main member at its end is zero, whereas the intensity of the stress in the gusset plate at the same point is a maximum. At some intermediate point the intensity of the stresses in the gusset plate and in the main member are equal. Designate this point as the working point. If the main member is in tension, the portion of the member between the working point and the edge of the gusset plate will elongate more under stress than the corresponding part of the gusset plate, and therefore the rivets at the edge of the gusset plate will be strained more than the rivets at the working point. Likewise the portion of the main member between its end and the working point will be stressed less than the corresponding portion of the gusset plate, and therefore the rivets at the end of the main member will be strained more than the rivets at the working point. That is, where we have assumed the stress to be uniformly distributed among the rivets we know that it is not so distributed. It follows, therefore, that the maximum stress per rivet exceeds the quotient, total stress divided by the number of rivets. A comparison of the strain in a riveted joint for ordinary working stresses with the differences in the strains in the gusset plate and the main member, shows that the maximum stress per rivet may be very much greater than the average stress per rivet.

Since a riveted joint, at usual working loads, resists a stress by virtue of the friction between the pieces connected, and since a riveted joint takes a permanent set at loads much below the usual working loads, overload in a rivet subjected to a reversal of stress may cause the rivet to work loose and must be considered as a serious matter. This fact, together with the fact that stresses on rivets are not uniformly distributed, as assumed, but that the maximum may be much greater than the average, indicates that the number of rivets in the connections should be increased, especially for members subjected to a reversal of stress.

The uneven distribution of the stresses can only be reduced by reducing the lap of the main member on the gusset plate. This suggests the desirability of arranging the plates so as to put the rivets in double shear, thus reducing the number required, or, as has been suggested, of using a comparatively small number of large turned bolts instead of a large number of rivets.

The thickness required for gusset plates is another question that has no satisfactory answer. Some designers claim that a truss can be assembled with only 20 per cent for details, other designers use 50 per cent. In general the details for a railway bridge truss are about 33 per cent of the main members. As gusset plates comprise a large part of the details, the percentage of details depends largely upon the thickness of the plates. The thickness of the plates can not be computed except that the strength of the rivets in bearing should develop the strength of the rivets in shear. Usually the plates are thicker than is necessary to meet this requirement. The outstanding fact is that the weight of a truss can be materially altered by changing the thickness of the gusset plates, and we have no positive evidence determining the proper thickness. We have opinions, of course, but the man who assembles trusses on 25 per cent details is positive that any one using more than 25 per cent is wasting material, whereas the man who uses 50 per cent details is positive that any less than that endangers the structure.

In general, in judging of the merits of a connection, it is well to bear in mind that the main member is the major part and the connection is the minor part, and that it is not good logic to jeopardize the major part to save a little on the minor part. Furthermore, if the life of a bridge is determined by the time it will wear as a machine, it will be the connections that will wear instead of the main members. For this reason also the connections should be stronger than the members.

The whole question of the strength of connections is a field ripe for experimental investigation.

The illustrations that have been used have been taken from bridges, but the designer of buildings must meet similar conditions. Moreover, the examples cited illustrate only a few of the many uncertain quantities which enter the design of a structure.

ADVANTAGES TO BE GAINED FROM A MORE EXACT KNOWLEDGE OF THE BEHAVIOR OF STRUCTURES

The object in having a more exact knowledge of the behavior of structures is not either to use more material or to use less material, but rather to use the material so that it will do the most good. The structure must be safe, but it should also be economical. The advantages to be gained are economy and safety.

Greater economy in design can be obtained from an increase in knowledge in two days. If the stress in a member is determined with a high degree of accuracy, what is usually known as a factor of safety is in reality a factor of safety, whereas if the stress is not known, what is termed a factor of safety is largely a factor of ignorance, and the member is made unnecessarily large because of the uncertainty in regard to the stress. If the stress in a member is determined with a ligh degree of accuracy those types of structures and types of details will be selected for which the secondary stresses are a minimum, and either the safety of the structure will be increased or the cost of the structure will be decreased.

The most important advantage of an increased knowledge relative to the behavior of a structure in the increase in the safety of the design. Shakespeare says that fear springs from ignorance. To the engineer the unknown is the source of danger.

With very few exceptions, failures have been due, not to error in the calculations, but to error in the assumptions upon which the calculations are based. It is the failure of the material to have the properties with which it is credited, or the failure of the structure to resist a stress in the manner assumed, that causes all failures. Design is not solely a mathematical process. Every equation used is based upon assumptions. These assumptions, the foundation upon which our structure stress analysis is built, need constant inspection.

Structural engineering has made wonderful progress both in design and manufacture. But it is only by keeping before us the importance of the factors that are neglected that progress is to continue. To the man with an inclination for research work, whether engaged in practicing engineering or in university work, structural engineering is a wonderfully attractive field.

Food and Work

MUCH of what we know about our food has been derived from empiric experience handed down to us for ages. With the knowledge so acquired the human race, under normal conditions, got along comparatively well. In consequence, little attention was given to the scientific investigation of food problems until within the last half-century. Even now the lack of knowledge amongst well-educated people of the composition of foods and their relative nutritive values may not inaptly be compared with that which prevailed in respect to fresh air and ventilation before the discovery of oxygen and its use in breathing. It is not the purpose of this article to trace the various steps by which the gaps in our knowledge of food requirements have been filled. But one important discovery of no distant date deserves mention, namely, that each of our foods has its own particular value in respect to the production or output of work by the human body. A consideration of this aspect of food problems is nowadays of vital consequence to a people, which for the most part is a work to maintain its national existence.

But before referring to the energy-value of foods it should be remembered that the human body, in common with that of every living being, suffers continuous loss every day, due partly to wear-and-tear of its structure, partly to the performance of work, and partly to the production of heat for maintaining body warmth. This loss is made good by food, which therefore has several functions to fulfil, namely, to supply material for structural repair and in early life for growth, to provide energy for the performance of work, and, lastly, to furnish fuel for the maintenance of heat.

FOODSTUFFS

If an ordinary suitable diet be examined it will be found to contain certain classes of substances known as "foodstuffs," which have been proved to be necessary for nutrition. The first of these is exemplified by the lean of meat, the white of egg, the casein of milk, the gluten of flour. These are the proteins or albuminous foodstuffs, and it is only from one or other of these that nitrogen can be obtained to nourish the animal body. A certain amount of this class of food is therefore indispensable, and cannot be replaced by any other. But proteins are not all equally valuable. Some, such as gelatin, can supplement other proteins in supplying nitrogen, but by themselves are unable to sustain life; they are "inadequate" proteins. Others, such as the gliadin of wheat and the legumin of peas, are "adequate" to provide for maintenance, for energy, and for heat formation, but not for growth. Still others, such as the casein of milk and the glutenin of flour, are adequate for all these purposes, and for growth as well. Inadequate proteins lack one or more ingredients indispensable for nutrition. It is desirable, therefore, to vary the diet in order to secure a sufficient amount of adequate proteins.

The second class of nutrient substances comprises the fats and oils. These serve to supply energy which may be transformed into either work or heat, or partly into one and partly into the other. A certain amount of fat is necessary for health, but it is, to a large extent, replaceable by foods of the following class, namely, starches and sugars. Fats, like proteins, are derived from both animal and vegetable sources. Most foods contain fat in a form not visible to the naked eye.

The third class of substances in our diet, necessary for healthy nutrition, includes starch and sugar. These are known as carbohydrate foodstuffs. They are used solely for the performance of work and the production of heat, and are not stored as such to any extent in the human body. But if taken too abundantly or by those who lead sedentary lives they may be converted into fat, and are then stored up in this form.

A suitable diet must also supply a certain amount of mineral salts, such as phosphate of lime, and many others which enter into the composition of flesh and other body tissues. These are contained in most articles of diet in sufficient quantities, and hence (with one exception, namely, common salt) are not specially added to the food.

Lastly, it has been found by recent investigations that to maintain health our food must contain certain accessory substances, the nature of which is not fully known, but the lack of which brings about diseases, such as polyneuritis or beri-beri, scurvy, and possibly rickets. These substances, known as "vitamines," are present in minute but sufficient quantities in most natural foods. They are, however, liable to be removed in the process of manufacture or destroyed in the preparation of foods. Hence it is desirable that the daily diet should include some raw food, such as salads or fruit. All vitamines are not killed by the cooking of food; some are undoubtedly thermostable.

Graphical Control*

On the Exception Principle for Executives

By Frank B. Gilbreth, Mem. A.S.M.E.

We have stated many times that the greatest waste in the world to-day is from unnecessary, inefficient and ill-directed motions. Many people think that this statement refers only to such activities as those of the bricklayer, the shopworker and other kinds of mechanics and manual workers. It does refer to them, but by no means to them only. It refers to the activity of every one and, by no means least, to that of managers and all other executives.

To one trained in the sciences of management and motion study, nothing is more ridiculous and pitiful than the average executive when he tries to enforce new motion methods on those farthest below him on the industrial scale, while he at the same time commits nearly all the motion wastes in his own personal work. The personal work of the executive should consist, as much as possible, of making decisions and, as little as possible, of making motions. General recognition of this fact has resulted in the common practice of assigning to the executive one or more secretaries, or clerks, to relieve him of certain parts of his work which involve more motions and less important decisions than that part of the work retained by the executive. This procedure varies in degree according to the kind of work done by the executive and how well he realizes the possibilities of eliminating waste through the use of the "exception principle" in management.

Many executives have used this principle unconsciously, and have carried the practice to the extent of making decisions result from a study of sheets of figures on which the "amounts which are less than those of the last fiscal period" are carefully noted in black ink and the amounts which are greater are carefully noted in red ink. Written causes of the fluctuations are conspicuous by their absence.

Some executives, but fewer by far in number, are furnished with charts which show by means of comparable curves the increase or diminution in outputs, costs, overhead expenses and, in comparatively rare instances, even in results as compared with budgets. As compared with an organization which has no cost system, such a recapitulation, even in the form of an "expenditure system," and such cost statements and graphical charts are a great step forward. No cost system nor chart system, however, can be considered really satisfactory unless it fulfills the following requirements; i. e., it must determine and show:

1.—What the quantities of individual outputs should be. Prophecies of outputs.

2.—Prompt records of individual outputs.

3.—What the costs should be. Prophecies of costs.

4.—Prompt records of costs.

5.—Causes of fluctuations and deviations of outputs and costs from prophesied outputs and costs.

The executive may have much to do with originally determining items 1 and 3; but after the computations of 1 and 3 have been completed, he can best attack the problem of enforcing items 2 and 4, and also of determining 5 by the use of graphical charts. He should provide himself with charts which will tell him how promptly such records of output and cost have been made; or, in other words, how much time has elapsed between the completion of the output and the recording of it and its attending costs.

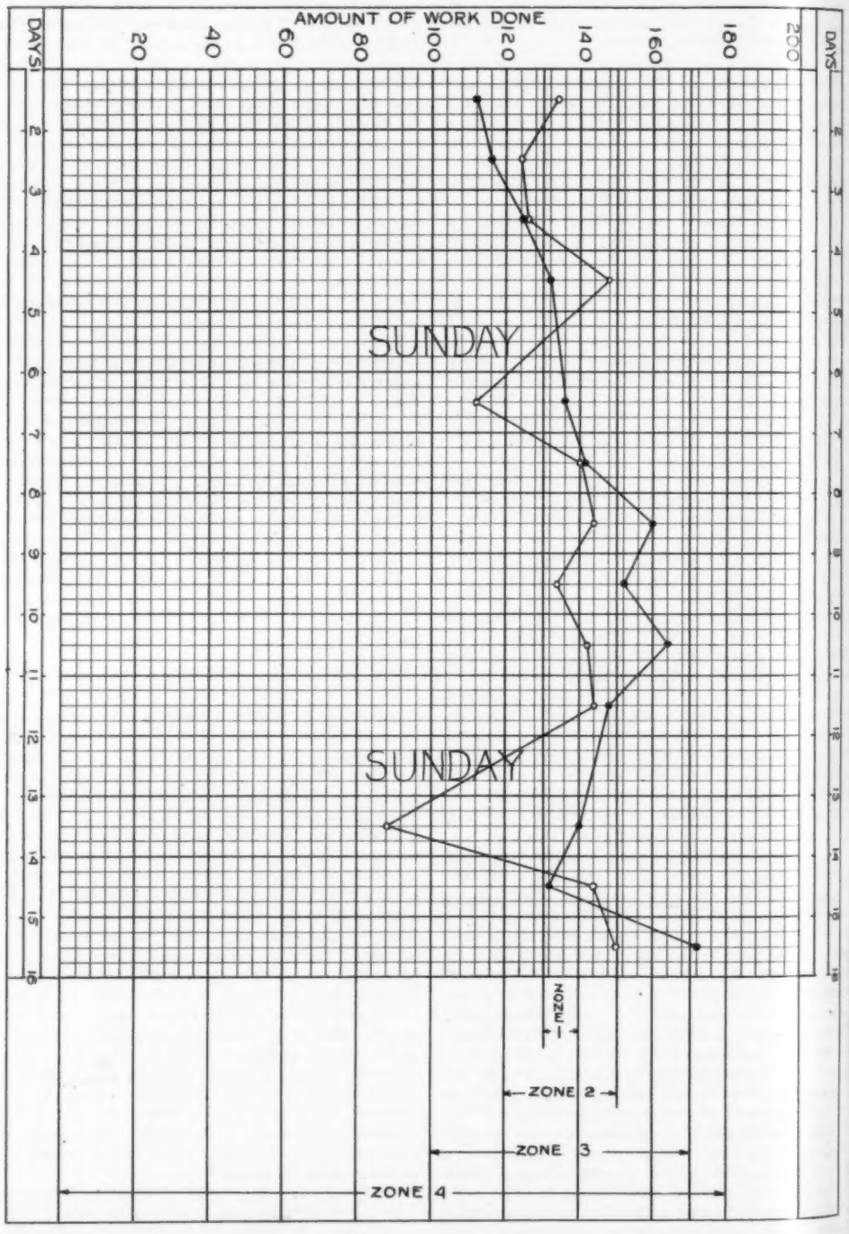
A long experience has shown us that the by-products of a properly operated chart system are even more valuable than its direct product. We find that the psychological effect of the variable "promptness" itself makes the curves representing outputs and costs fall more nearly in the proximity of the established forms and locations prophesied on the charts. Such charts give the executive and his colleagues accurate measured information of deviations from class in all departments. The motions that an executive would expend in getting information by such old methods as, for example, walking through the works to see with his unreliable eyes conditions which are not typical, partly owing to his presence, bring results of little value compared with the results that can be obtained by the same amount of time and motions concentrated on those facts and conditions which cause the great fluctuations from the desired output.

While this fact is generally recognized, the number of installations of chart departments throughout the country is increasing with surprising slowness. Even in those organizations where there is a satisfactory cost system supplemented by charts with curves—showing results as compared with expected conditions and ideals—the executive too often finds himself flooded with charts. Then, being human, he postpones studying them. As a result, many benefits which come from *promptly* making records of outputs and costs are lost by his delayed action. It is here that the "control on the exception principle" plays the important part.

It is obvious that the foreman, or other functionary, should see *promptly* all the records of output in his particular department after they are achieved. In most cases he will be able to handle his duties still more satisfactorily if he also sees the costs of the outputs of his department. The time of the over-foreman, however, who may have several foremen and departments under him, is too valuable to have him, also, examine with care *all* the records of all the men under him. Consequently he should be furnished with information in concise form, in order that as little as possible of his time may be taken. This has often been furnished him in the form of "averages."

Ordinary averages have their use. *Progression averages* are, however, more valuable, because they show the *trend of progress and efficiency*. It sometimes pays to make ordinary averages, but the value of examining such ordinary averages is slight compared with the benefits which result from concentrating the same amount of motions and attention on those individual cases that brought the average away from the ideal. A case of "bad average" may be the excuse for putting the foreman "on the carpet," but the results of this do not compare with the good results that are derived from having the over-foreman investigate promptly the case or cases that spoiled the average. For here the over-foreman, the foreman and the workman have every opportunity to secure super-co-operation, and the over-foreman can give that constructive criticism afforded by reason of the experience and knowledge that he should and probably does possess.

Moreover, the decisions of the over-foreman can be made more quickly, for he has the information which comes from locating the trouble accurately. Instead of teaming out the foreman or the workman, he will find, from the causes marked on the chart, that the



*A paper presented at the annual meeting of the American Society of Mechanical Engineers, December 6, 1916, New York.

worker's low output is due to lack of the proper tools; to his not having been furnished with tools in standard condition; to the routing system having failed to give him proper materials in the right quantities, in the right sequence and at the right time; to something which has gone wrong with the equipment or surrounding conditions; to the man's not having been properly instructed; to there having been an unwise selection of the man or the machine, or both, for the particular job. Whatever the cause, the tendency toward killing co-operation by having a brainstorm prior to an investigation will be gradually eliminated.

The worker, also, is more careful not to do anything which is not expected of him, because he knows that the exception will surely be noticed by the executives higher up and will interfere with his chances for promotion or transfer to work of a more desirable kind. Knowing that they will be investigated properly will create a tendency on the part of the foreman and the workers to co-operate with others whose work affects theirs, or who in turn may be investigated. This co-operation becomes general and sooner or later becomes a habit.

Now the time of the executive next above the over-foreman is still more valuable than that of the over-foreman, and so on up to and including the managing director or president. No executive should make a routine motion of handling, turning over or examining charts containing data, either normal or with considerable deviation from class, where the *causes* of the deviation can be handled properly by those in lower executive positions. The exclusion of such cases can be obtained by having the executives determine *zones* on the charts, it being understood that as long as the points fall within the zone he is not to see the charts unless he specially requests to see them. He is, however, to have sent to him, for initialing, any chart having a point that falls outside the excluded zone.

In other words, he sees the charts on the "exception principle." So long as everything goes right, as long as points do not fall outside his zone of exclusion, he is not disturbed. When they do fall outside, he needs to and does see them. He can easily make other standing orders, in cases, for example, where the line is a certain distance away from the normal for a certain number of days in succession.

An executive of any class will find it beneficial to see exceptionally large cases of deviation on the desired side of the line so that he can recognize and appreciate and take a personal interest in cases of unusual efficiency. It is through such cases that he gets in touch with unusually good methods. This is a check on the Time Study Man's work. It also gives the executive valuable opportunities for proper managerial decisions in cases of the selection of candidates for promotion under the Three Position Plan of organization building. The curves showing progressive averages of departments may be examined at times farther and farther apart, these intervals to be determined in each particular case by the favorable or unfavorable comparison of records of such averages showing outputs and costs with the prophesied outputs and costs. The executive is thus relieved later of work which is necessary at first, but which is not necessary when the particular case is running satisfactorily.

It is impossible to prophesy with accuracy what the amounts of outputs and costs should be without motion study and time study. But once these have been made and the actual outputs and actual costs approximate those prophesied, the high executives should devote very little time indeed to inspecting this class of charts. Instead they should spend their time on other work, other departments and more important things where their supervision will bring more valuable results.

It will be seen that these *"Output, Cost and Causes Charts,"* with the exclusion zones, enable the executive to eliminate the motions required for general oversight and inspection until a place on a chart is brought to his attention where he can actually help those below him and furnish them with better instructions for handling their work more efficiently; or for making such changes as will naturally result in promotion or the selection or shifting of individuals better fitted to do work elsewhere. The possibilities of relieving the executive of unnecessary motions and of enabling him to be more efficient in his own work are not exceeded in the case of any manual worker.

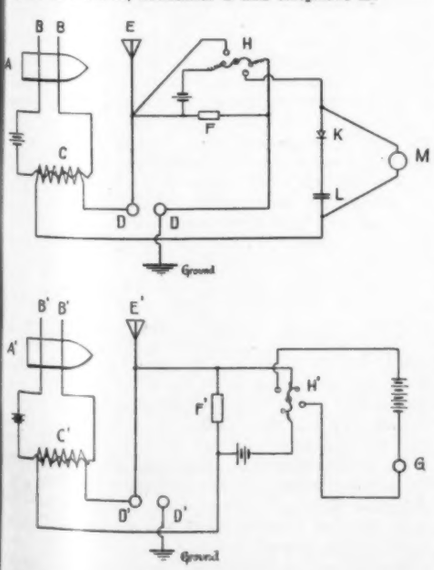
This system of Graphical Control for executives on the Exception Principle has another by-product of great value to us as a nation. It will provide us with better executives—a subject of great importance, not to be treated lightly, for when the executive tells those below him "how to do it," the effect of his decisions, the value of his judgment, his efficiency as an executive, his standing as a leader of a department or an undertaking, or of the destinies of those workers who have thrown in their lot with him, can be measured and compared just as efficiently and accurately as can the output of any other workers.

In the great commercial war which will soon be upon us, as a nation and as individuals, we need better leaders. Our national and individual prosperity depends upon how efficiently we are led. This method of Graphical Control on the Exception Principle provides for the personal contact, the square deal and the recognition of the efficient man, and at the same time saves the motions and time of the executives.

Testing Speeds of Projectiles by Wireless Waves

A well-known Paris wireless constructor, M. Ancel, brings out an interesting method for measuring the speed of projectiles by the use of wireless waves which we illustrate in our drawings. In general, the method consists in the use of a sending station and a distant receiving station, the projectile being fired between these two posts. As usual in all electric speed taking methods, the projectile is fired through a pair of frames covered with a network of fine wire located just in front of the cannon, and as each frame is connected in an electric circuit, the projectile establishes a contact when it passes through the pair of frames, and this contact puts current into an electric device which records the instant of starting. At the far end is located a target of the same kind, which records the moment of arrival. Such a system is operated by wireless on the following plan:

The home station has the parallel screens *B B* for the projectile *A*, and the circuit includes a battery and an induction coil *C* which works in connection with the spark gap *D D* and the aerial *E* and earth. A coherer *F* is mounted so as to send current into a sensitive relay *H*, which serves to close a circuit containing the crystal detector *K*, condenser *L* and telephone *M*.



The distant post has one portion disposed on the same makeup as the former, but the rest of the connections include the relay H^1 which closes a circuit containing a battery and a suitable recording instrument *G*.

When the projectile is fired, it makes contact at the frames *B B* and this closes the circuit of the battery and induction coil upon the spark gap, causing a wireless signal in the usual way. This signal is received at the distant station and the coherer F^1 closes the circuit of the relay H^1 which in turn puts current on to the electric recorder *G*, and this records the exact instant of the projectile's start. When the projectile reaches the second station, it enters the target and sets up wireless waves in the system D^1D^1, E^1, and these waves act upon the coherer F^1 at the same post so as to again operate the relay H^1 and produce a second record upon the instrument *G*. Observation of the first and second records on the instrument thus gives the speed of the projectile.

The object of the coherer and the other devices at the first or sending post is to make use of a back signal coming from the distant post in order to observe whether the projectile has arrived at this point. The coherer *F* might answer this purpose in connection with a telephone in order to receive the signal sent out from the aerial E^1 of the distant post on the arrival of the projectile, but in practice it is better to use a crystal or an electrolytic detector *K* which is included in the circuit shown. The coherer thus operates the relay and this throws in the second circuit of the crystal detector condenser and telephone so that the signal can be heard, and at the same time the crystal detector is preserved from harm, as it is only placed in circuit at the exact time when it is used, and the rest of the time it is cut out.

The Chemistry of Wine "Chaptalization"

The process of neutralizing the acid in wine by the addition of calcium carbonate has long been known in France by the term chaptalization. The term as well as the custom has made its way in Germany because of its convenience, and both name and practice have been legally authorized. The poor harvests of recent years have led to the extension of the practice, so that a German chemist, Dr. Theodore Paul, has considered it advisable to make a thorough study of the chemical reactions involved in the process, with a view of replacing previous empirical methods by those of scientific exactitude. He reports the result of his studies in *Di Umschau*.

The process consists in the addition to must or young wine of pure precipitate calcium carbonate-marble dust is also used in France. It has heretofore been supposed that because of this addition an equivalent neutralization of the wine took place, by means of the transformation of a corresponding portion of the free and half combined acids into calcium salts, the carbon dioxide formed at the same time escaping more or less completely. Dr. Paul, however, finds the process to be more complex in its reactions, saying:

There is, in fact, in this neutralization a retrogression of the titresable acid corresponding in degree to the amount of calcium carbonate added. As demonstrated by Th. Paul and Ad. Gunther, the *degree of acidity of the wine is identical with the concentration of the therin contained hydrogen ions*, and is dependent upon the equilibrium of the salts and acids contained in the wine.

Consequently the "de-acidification "of the wine with calcium carbonate is no simple neutralization process, but is the result of a series of chemical processes and alterations of equilibrium. To obtain a clear knowledge of this matter was the object of the present very extensive investigations, wherein I had the grateful aid of my assistant, Dr. Heinrich Zirkel. It was first determined that the precipitate formed in the wine after the adding of calcium carbonate consists essentially of neutral calcium acetate. Hence the study of this salt was the starting point of the search.

The next step was the experimental study of the chemical equilibrium between acetic acid and calcium carbonate in aqueous solution and in weak alcoholic-aqueous solution, and the application to the wine of the knowledge thus gained. In spite of the manifold variety of the chemical processes involved, the chemical equilibriums can quite readily be represented schematically and can also generally be controlled by calculation.

The experiments show, among other things, that in the neutralization of an acetic acid solution with calcium carbonate *the degree of acidity diminishes at the beginning with much more rapidity than it does later*. This is due to the formation of primary and of seeondary acetic acid ions, which remain in the solution until the formation of the precipitate of calcium tartrate, and exert their retarding influence on the dissociation of the acetic acid.

The titrimetric acid content of the solution on the other hand diminishes steadily and in direct proportion to the increase of calcium tartrate, as was theoretically predictable, and as could be proved also by continued titrations.

Finally there was undertaken gradual neutralization of wines of pure nature, in accordance with these investigations. Here likewise there was exhibited a similar accelerated decrease in acidity at the beginning, having an unmistakable connection with the formation of the precipitate. The same thing was observed in gross by Von der Heide and Baragiola.

In consequence of the alcohol content, which diminishes the solubility of the calcium tartrate, this separation out begins earlier, and on this account the retardation of the hydrogen dissociation in the wine is not so noticeable at first, and the fall of the acidity curve is not so steep a descent as in the aqueous acetice acid solution.

Economy in Electric Cooking

It is claimed that the average shrinkage of meats when cooked in an ordinary coal heated range is 35 per cent, while the shrinkage in an electric oven is but 10 per cent.

The Movements of the Earth's Pole*

Investigations of the Nature and Course of Its Wanderings

By Col. E. H. Hill, F.R.S.

More than a century ago it was shown by the mathematician Euler that if the axis round which the earth was rotating were not coincident with the axis of figure, which latter in the case of a spheroidally flattened earth is the shortest axis that can be drawn, the axis of rotation will revolve about the axis of figure in a period which, upon certain assumptions, can be precisely predicted. The time of one revolution of the pole of rotation around the pole of figure depends only upon the shape and degree of elasticity of the earth. In Euler's days the supposition that the solid earth had any appreciable elasticity was so far outside the range of experience that it was not considered by him. He calculated the period of the polar rotation on the assumption that the earth was perfectly rigid, and showed that this period would be about 305 days.

If we determine the latitude of a point on the earth's surface by observations of the stars, we are in effect measuring the angular distance between the axis of rotation of the earth and the vertical line, or line through the zenith, at the point of observation. If, now, this axis of rotation moves, the observed latitude of the place will change, and if we prolong the observations over a sufficient time, we ought to find that this observed latitude fluctuates backwards and forwards about a mean value with the same periodicity as that in which the earth's pole of rotation moves round the pole of figure.

Every observer who is engaged in making observations to determine the precise positions of the stars, a class of observation which up to a few years ago occupied a very large fraction of the time and energies of astronomers, is actually continually determining and redetermining the latitude of his instrument. There is thus an enormous mass of latitude observations available for examination, and it should prove a not too difficult task to analyze these with the object of detecting a periodic variation. Two causes, however, militated against success in this inquiry: first, the very small magnitude of this variation; and, secondly, the fact that the earth is by no means rigid, and hence that the true period of the precessional rotation differs very substantially from the Eulerian period of 305 days.

All the earlier attempts to find evidence of this variation were, in fact, hampered by this preconceived notion of the ten-month period; the observations were carefully scrutinized with the view of detecting it, a process, as we now see, foredoomed to failure. It would be a useless task to recount here the various attempts that were made. Two of these, however, I should not like to pass over without notice, those of C. A. F. Peters, at Pulkowa, and Clerk Maxwell in this country.

Peters in his great and classic memoir on the parallax of the fixed stars devoted one section to a discussion on the variability of the latitude in a ten-month period. He found that the actual variation derived from the observations was of so minute a magnitude that it was well within the limits of unavoidable sources of error, and he therefore concluded that if there was any separation of the two poles it was too small to be detected by observation.

Clerk Maxwell examined the Greenwich observations of Polaris in 1851-4, and thought he found some small indications of maxima at about ten-month intervals, but he considered the results as very doubtful, and that more observations would be required to establish the existence of so small a fluctuation.

Substantially the same result was derived by other inquirers. Astronomers were therefore satisfied, up to the year 1884, that the earth's axis of figure was so nearly coincident with its axis of rotation that the difference between the two was inappreciable to the most refined observations. All methods of observation and all principles of the reduction of observations, both of astronomers and of geodesists, were tacitly based upon the idea of absolute coincidence between the two axes.

In 1884 the subject was independently reopened by two men—Chandler in America, and Kustner at Bonn—and entirely fresh light was thrown upon it. Their work was simultaneous and quite independent. I will take Chandler's first.

In 1884–5 he took a thirteen-month series of observations at Harvard with an instrument of his own devising, to which I will revert later. These observations showed a progressive change in the derived latitude, which appeared to him of a greater magnitude than could be accounted for by any instrumental errors. He, however, hesitated to ascribe it to a real change in the latitude without further confirmatory observations, which he could not then make. He therefore put these observations aside, and was, six years later, drawn to re-examine them by the publication of some of Kustner's results, which were also only explicable on the hypothesis of an actual variation in the latitude of the place of observation. It was, however, quite obvious to Chandler that his series of observations contained no warrant for an Eulerian period of ten months, and he therefore, to quote his own words, "deliberately put aside all teachings of theory, because it seemed to me high time that the facts should be examined by a purely inductive process; that the nugatory results of all attempts to detect the existence of the Eulerian period probably arose from a defect of the theory itself, and that the entangled condition of the whole subject required that it should be examined afresh by processes unfettered by any preconceived notions whatever." This bold rejection of theory and appeal to observation alone was rewarded with immediate success, and Chandler was able to show that his observations of 1884–6 contained unmistakable evidence of the rotation of the one pole about the other in a period of, not 305 days, but 428 days. Wherein, then, lay the deficiency of Euler's investigation? As already hinted, this arose from the assumption of rigidity, and it was shown first by Newcomb, and afterwards, more completely, by Hough, that the 428-day period was fully in accord with a degree of elastic yielding of the earth quite consonant with probability. Hough showed that if the earth were as rigid as steel the period would become 440 days; that the actual period is somewhat shorter than this means that the earth as a whole is decidedly more rigid than steel, a result which accords perfectly with other known phenomena which depend upon the earth's elasticity, such as the rate of propagation of earthquake waves.

Immediately following on this initial success Chandler undertook a prolonged and most laborious examination of old observations and reached results which have not completely borne the test of subsequent review. He was confident that the whole movement of the pole might be explained as the superposition of two rotations, one circular, with a 428-day period, and one elliptical, with a period of a year. He thought, further, that there was evidence that the longer period had varied in past times, and that in 1770 it was less than a year. This last result was traversed by Newcomb, who showed its extreme improbability. While fully bearing in mind the lessons of past experience as to the unwisdom of relying too closely upon pure theory, we cannot resist the conclusion that to accept any large change in the 428-day period within recent years would be to set aside the whole dynamical justification for accepting this period as a reality, it being quite impossible to admit that the elastic constants of the earth can be subject to any appreciable alteration within such time as a century or so.

As regards an annual period, we should now prefer to say that, while there are doubtless seasonal transfers of material upon the earth, such as the accumulation and melting of Arctic ice, which may produce a movement of the pole with an approach to a yearly periodicity, the part of the movement due to a true annual period is very small, and is quite masked by large, irregular disturbances. We shall be on safe ground if we say that the observed polar motion is compounded of a precessional rotation in a period of something very near 428 days at an average distance of 20 feet from the mean pole, with an irregular movement superimposed on it; this irregular movement having sometimes the effect of modifying the rate of precessional rotation and sometimes of changing its amplitude—that is to say, altering the distance between the pole of rotation and the mean pole—according as it is acting parallel to, perpendicular to, or at any intermediate angle to the direction of the precessional rotation. I shall revert to this question later, and show how it is possible by a simple graphical construction to separate out this irregular motion and construct a diagram of it which should be helpful in elucidating its cause.

While it is thus to Chandler that the credit of discovering the 428-day period should be ascribed, it is to Kustner that we owe the first real proof that there is an actual variation in the latitude of a point upon the earth.

Kustner's observations were made in the same years as Chandler's, 1884–5, and were designed to determine the constant of aberration, a class of observation identical with those which would be used to determine the latitude of the place. Upon reducing these observations the results were at first sight anomalous in that they gave an impossibly small value of the aberration constant. The anomaly was not due to any instrumental cause; it could not be due to any seasonal change in the refraction, as the morning observations of 1884 were not accordant with the morning observations of 1885, nor could it be explained by any possible error in the proper motions of the stars. Kustner was thus enabled to state positively that the latitude of the place of observation had actually changed. It must be admitted that the years 1884–5 were particularly favorable ones, and that both these astronomers were in a sense lucky in having chanced upon them. The movement of the pole happened at that time to be exceptionally rapid. I do not, however, mention this as detracting in any way from the merit of their achievements; they deserve to be remembered as simultaneous but independent discoverers of this important and interesting phenomenon, and should be honored, Chandler especially for his courageous rejection of mathematical theory, and Kustner for the very high skill and exquisite refinement of his observational's work.

The importance of Kustner's discovery was at once recognized upon the Continent, and a proposal was made to the International Geodetic Conference to establish a chain of stations for carrying on a series of simultaneous observations and thus deducing the true law of this latitude variation. The suggestion was soon carried into effect. Six stations were chosen, all at the same latitude, 39.1° N.—Carloforte, in an island close to Sardinia; Mizusawa, in Japan; Gaithersburg in Maryland, and Ukiah in California—all new stations, where special observatories had been built for the purpose; a new observatory, established by the Russian Government at Tschardjui, in Russian Asia; and the existing observatory at Cincinnati. The reason for selecting stations at the same latitude was that identical sets of stars could be observed at each place, and thus any errors due to defective knowledge of star places are similar for all. These began work in 1899. Later, two stations in the southern hemisphere, at latitude 31.5° S.—Bayswater in Western Australia, and d'Oncatwo in the Argentine—were added.

The results were reduced and discussed by Professor Albrecht at the Geodetic Institute, Potsdam, and published with a diagram showing the actual polar movement as deduced from the mean of the observations at all the stations from time to time.

It was not long before these observations yielded a result of the highest interest. The observatory which devoted itself most whole-heartedly to the work, and at which the observations were most extensive and most precise, is that in Japan. This was under the able direction of Professor Kimura. By a searching discussion of the whole series of observations he showed that they became far more consistent if a new term were introduced into the expression for the latitude variation, this term having an annual period, but being independent of longitude and having the same value for all the stations at the same date.

It will be readily seen that this term differs completely from those we have been considering hitherto. It is not a shift of the earth's axis or a movement of the pole of rotation; as it affects all places along a parallel of latitude equally the pole evidently does not move, but something which has an effect exactly the same as if the center of gravity of the earth were shifted a few feet up and down, northward and southward, from its mean position.

The great difficulty in elucidating the Kimura term lies in its extremely small magnitude and in the consideration that there are so many possible sources of error affecting observations of this class which might have annual periodicities that their separation and evaluation are extraordinarily complicated questions. This is not the place to attempt any complete discussion, but a mention of some of the lines along which a solution has been sought may detain us for a few minutes.

The magnitude of the term at the latitude of 39° is

*Discourse delivered at the Royal Institution, republished from *Nature*.

about 6-100ths of a second of arc, or 6 feet on the earth's surface. It has the same value and phase for every station on the same parallel and is zero on about March 9th and September 12th, and maximum and minimum on June 10th and December 10th, *i. e.*, about ten days before the equinoxes and solstices respectively. It cannot be accounted for as a real shift of the earth's center of gravity. It is true that in the alternate melting and accumulation of ice and snow at the two poles we have a periodic factor at work which does do this, but the amount is far too small. It was pointed out long ago by Van de Sande Bakhuysen that to fit in with the observed value of this term the apparent path of the center of gavity must have an amplitude of 3 meters, which, if translated into terms of polar ice, would mean that a cap of ice one kilometer thick and 244 square degrees in area would have to form and disappear each year. This is obviously quite impossible. There are certain possible errors in the accepted values of the proper motions and parallaxes of the fixed stars which might produce an apparent variation in the observed latitude of this nature. As all parallaxes are based upon differential measures we cannot with certainity say that such errors are impossible; we can only say that they appear to us very unlikely, and that, if they were actually proved to exist, our ideas of the stellar universe would be profoundly modified.

If there were a yearly term in the refraction which had the effect of a periodic change in the apparent zenith we should get a corresponding periodicity in the observations. If, for example, there were a solar atmosphere, even of a quite tenuous nature, which extended into space beyond the earth's orbit, we should get a seasonal change due to the varying angular distance of the sun from the zenith of the place of observation. An atmosphere which could bend rays of light to the requisite amount, though undoubtedly extremely rare, would, however, be dense enough to offer an amount of resistance to a planet, or *a fortiori* to a comet, inconsistent with observed facts. It is, however, quite possible that the changing declination of the sun may curve or tilt the mean isobaric surfaces in the upper atmosphere in such a way that the apparent zenith moves north and south about its mean value, and that it is to this cause we owe the greater part, if not the whole, of the Kimura term. Such a displacement of the isobar is highly probable, and the phase times of the latitude variation—nil at equinoxes, maximum northward at summer solstice, and maximum southward at winter solstice—fits in perfectly with this explanation. The observations made in the southern hemisphere should form a crucial test. If this is the true cause the apparent latitude of a southern observatory will be shifted in the same direction as that of its northern counterpart, *i. e.*, northward in June and southward in December. We have only a short series of observations from southern stations, but so far as they go they appear to conform. There is thus fairly strong evidence in favor of this explanation.

It must not, however, be assumed that the matter is settled beyond dispute. More observations are necessary, and especially observations at widely different latitudes. The international stations are, as to the northern ones, almost exactly on a parallel, and, as to the southern ones, on a parallel differing only by 7½° from the northern. This uniformity, highly advantageous for securing a precise record of the motion of the earth's pole, is disadvantageous for solving the riddle of the Kimura variation, and other places should join in the attack. Unfortunately the observations are very laborious and require the almost exclusive attention of an observer. There is, therefore, a very real want of an instrument which shall demand something short of the whole time of a skilled astronomer. With this object, and also with the intention of eliminating certain sources of error, instruments of new form have been devised. A short account of these will be of interest.

I shall not here attempt any description of the methods of observation used. It will be sufficient to say that, as what we want to find is the direction of the zenith at the place, all methods ultimately depend either upon a level, giving us the horizontal plane, or upon a plumb-line, giving us the vertical, and that of these two the level is the one that has almost exclusively been employed by the astronomer. The level is an instrument capable of a high degree of precision, but it has the disadvantage of being very susceptible to temperature changes, and, as both the glass tube of the level and the spirit with which it is filled are bad conductors of heat, it is impossible to ensure that it is at an even temperature throughout. Irregularities are thus produced which the reading of both ends of the bubble only partially eliminates. The mere fact of an observer standing near a sensitive level to read it may seriously vitiate its accuracy.

Some of these errors may be avoided, and such errors as are due to faulty reading of the level graduations by the observer entirely eliminated, by making the level an integral part of the instrument by floating the whole in liquid. The first application of this principle to an astronomical instrument was by Chandler, who carried out his series of latitude observations, already mentioned, with an almucantar, being a transit telescope floated in a trough of mercury. The name "almucantar" means a small circle of the heavens parallel to the horizon, and it will be sufficiently obvious that if the telescope can be set at any angle with the float, then as the instrument is rotated in the trough, or the whole trough itself is turned, the line of sight of the telescope will move round such a circle. With this instrument the stars are observed, not as in a transit circle crossing a vertical line, but crossing a horizontal circle of constant altitude. For convenience of calculation this horizontal circle would generally be selected as that through the celestial pole at the place. Chandler's instrument was purposely designed so as to differ as little as possible from the ordinary visual type, and must have been a most difficult instrument to use. The fact that he got such excellent results from it is no small tribute to his manipulative skill. The use of this form of instrument cannot be said to have found great favor among astronomers; there is only one example of it in this country, and, so far as I know, none on the Continent. The one we have is at the Durham University Observatory, and was designed by the present Astronomer Royal for Scotland, in coöperation with the late Dr. Common. It marked a very decided advance upon the earlier type. In two points specially, the screen of the floating part from wind disturbance, and the attachment of the eyepiece to the fixed part, the designers had the idea of a movable instrument, which a slight touch or a puff of wind would set vibrating to such an extent that no observation would be possible for a minute or two, clearly before them. The almucantar method of observation, meaning by this, not the use of a floating type of instrument, but the observing of stars crossing a horizontal circle, though appropriate for the particular class of observation we are here concerned with, those for determination of latitude, is not absolutely the best that can be used. To reduce every possible source of error to a minimum, particularly those due to refraction of the atmosphere, we want to observe stars as near the zenith as possible.

The floating principle has been applied with great success to a zenith instrument in the Cookson floating zenith telescope now at Greenwich, designed by the late Bryan Cooksan, whose early death was a great loss to astronomy.

It is a photographic instrument, with a telescope or camera tube attached to a circular float which floats in a ring-shaped trough of mercury. The angle between telescope and float can be altered so that it can be clamped to point either vertically upwards or at any angle, up to about 30°, from the vertical. It is used in the well-known Talcott method. A pair of stars is selected which cross the meridian within a few minutes of each other at nearly the same zenith distance, one north and one south of the zenith. The instrument is set so as to include the first star in the field, the lens is opened, and as the image of the star moves across the plate it traces a fine line or trail. After the star has crossed the meridian, the telescope is turned through 180°, leaving tube and float clamped in the same relative position, and the second star traces out its trail. The distance between the two trails on the plate, which is small if the difference of their zenith distances is small, when the appropriate corrections are applied, gives the observed difference of zenith distance of the two stars, and, therefore, the observed position of the zenith, and hence the latitude of the observer. By repeating the observation with a number of pairs of stars a very precise determination of the latitude is made.

Recently a zenith telescope, designed, not on the floating, but on the hanging principle, finding the vertical line by virtue of its free suspension in a gimbal ring, has been constructed, and would have been at work by now had it not been for the interruption caused by the war. Though it has thus not yet been tested by practical experience, a few words on it may not be out of place. The method of observation will be the same as I have just described, except that there is no arrangement for clamping the instrument at an inclination to the vertical; it is intended to be used only in the vertical position, and the angle covered by the photographic plate will be a few degrees from the zenith on each side. Exactly how far we can go from the zenith depends upon the qualities of the lens, and no confident statement can be made until this has been tested, but it is hoped that star trails perfectly sharp for measurement will be secured up to an angular distance of 3° from the center. This gives us as available for our purpose the stars over a belt 6° wide down to the sixth, and possibly the seventh, magnitude. The actual work of observing will be very simple, and will only mean that the whole instrument is rotated through 180° at certain prearranged times, and that the lens is opened after twilight and covered before the dawn. It would be possible for this to be done by mechanism controlled by a clock.

As the telescope hangs freely always in a vertical position, we entirely get rid of one of the astronomer's anxieties, the risk of error due to flexure or bending of his telescope, for though the tube can be made apparently very rigid, the excessively minute degree of bending sufficient to introduce appreciable errors is difficult, if not impossible, to avoid in a telescope which has to be used in different positions. Then, again, the errors due to changes of temperature inside or close to the instrument should almost disappear in this form. First, no temperature changes affect the suspension; so long as the body of the telescope remains undistorted the position of the true vertical in regard to the optical axis remains constant. Secondly, as the whole hanging part of the instrument is perfectly symmetrical about the vertical axis, with the trifling exception that the plate-carrier and photographic plate are not circular, but rectangular, no temperature change should distort the axis. Any distortion that can take place will, in fact, be the very small change of scale that will result from the difference in the expansion of the glass plate and the brass tube. Thirdly, it is possible, and in this instrument has been done, to enclose the whole in an outer case which can be made air-tight and kept at a constant temperature by a thermostat. In order to close the instrument in front it is necessary to have a plane parallel glass of slightly larger aperture than the lens. As this glass has to be worked with the same refinement as a lens, and as a plane surface is more troublesome to work than a curved one, this is rather a costly addition. Whether, as a matter of fact, it is worth while keeping the instrument at the same temperature, or whether it will be better to reduce the temperature change to a minimum by covering the whole with non-conducting material, and then apply the very small corrections necessary to the measurements made on the plate, is a question for experience to decide.

As a heavy hanging mass would be liable to long-continued vibrations when distrubed, a four-armed vane attached to a rod at the base is immersed in a dash-pot or bath of glycerine. This rod must be centred in prolongation of the vertical axis, otherwise the capillarity between rod and liquid will introduce a force deflecting the telescope from the true vertical. While it would thus appear that in this form of instrument most of the familiar sources of error are minimized, it is interesting to note the introduction of one possible cause of error, quite unfamiliar to astronomers, namely, the deflection that might be due to the attraction of the earth's horizontal magnetic force upon the hanging part. If the telescope-tube were, as is customary, made of iron or steel, this would reach a serious magnitude, and even if a proportion only of the suspended weight were of iron a perceptible deviation might result. It would, in fact, not be safe to allow this proportion to exceed one-tenth of the whole weight, and it therefore seemed better to exclude the use of iron or steel altogether. There is accordingly none, with the exception of the four thin flat pendulum springs which form the gimbal suspension.

In detaining you with these short descriptions of recently devised instruments, I may appear to have been wandering rather far from my subject, the wanderings of the earth's pole. You will, however, appreciate that in reality they follow very closely from it, being instruments designed with the special object of solving the particular problem we are discussing.

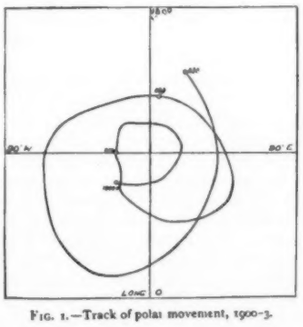


FIG. 1.—Track of polar movement, 1900-3.

We will now revert to the diagram of the observed polar motion, and I will indicate how it is possible to analyze this so as to separate the irregular movements from the more orderly fourteen-month precessional rotation. We are justified in assuming that this free precessional period is constant in duration and therefore determines the average rate of rotation of the pole of revolution. If, therefore, we take a diagram of the polar movement, which will naturally have its axes of reference fixed in relation to the earth, and convert

it into another diagram, showing the same movement, referred to axes rotating in the earth at the average rate of the precessional rotation, we obtain a graph of the irregular part of the polar path. If this irregular part

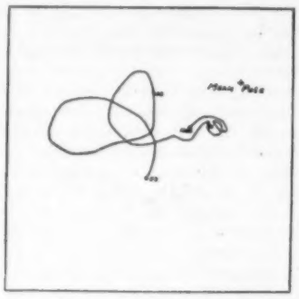


FIG. 2.—Same track referred to axis rotating in the earth with a fourteen-month period.

has any well-marked annual period, such period ought to be apparent on inspection of the converted diagram. In the actual diagrams obtained there seems little or no evidence of the existence of a yearly term.

We now take the second diagram, and by the well-known process construct its hodograph, the curve which gives us a measure of the amount and direction of the force which could have caused the movement recorded in diagram No. 2. This will still be referred to the

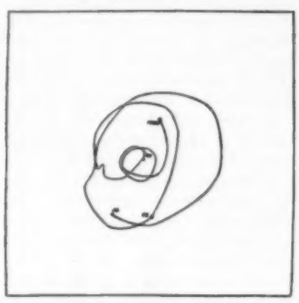


FIG. 3.—Hodograph of Fig. 2.

moving axes, so is not directly available for deducing the true direction of these forces in the earth. Before we can do this we must refer the diagram back again to axes fixed in the earth. Thus, finally, we obtain our diagram No. 4. which may be called the torque diagram, as it represents in direction and relative magnitude the torque or twisting force which has been acting upon the earth to produce the observed movement of the pole.

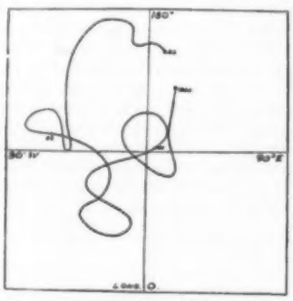


FIG. 4.—Hodograph referred back to axes fixed in the earth, or torque diagram.

The interpretation of such a diagram is a somewhat complex matter, and has not yet advanced far. The causes that seem to be at work producing the irregular shift are either movements of the earth's crust, slow or rapid, as in an earthquake; the transfer of Arctic ice from one point to another, or its accumulation and disappearance so far as this takes place unsymmetrically with respect to the earth's axis; and possibly extensive barometric changes extending over considerable areas.

Of these the transfer of ice is the largest factor and is probably the one to which most of the irrregular polar movement may be ascribed. An earthquake, even of gigantic dimensions, would have an almost negligible effect. The late Prof. Milne estimated that a very large earthquake might displace ten million cubic miles of earth through a distance of 10 feet horizontally or vertically. Such a vast cataclysm would only change the position of the pole by a few inches.

In conclusion it will be an act of natural curiosity to inquire whether there is any evidence of the amplitude of these polar wanderings having been greater in past times than at present, and whether there is any likelihood of their being greater in the future. To both these questions the answer is "No." The axis of rotation is always kept near the axis of figure by internal friction, and it would require a large change in the distribution of mass to move the axis of figure very far.

As regards the future, the probabilities point still more strongly in the same direction. Each shrinkage of the earth, whatever its immediate effect on the position of the axis of rotation may be, tends ultimately to bring it nearer to the axis of figure or principal axis of inertia, and therefore tends to reduce the average amplitude of the polar path. The distance of the pole of rotation from the mean pole will therefore gradually decrease as the world grows older, while at the same time, as the earth cools and becomes less elastic and more rigid, the rate of rotation will quicken.

Observations Upon Tropical Fishes and Inferences from their Adaptive Coloration*

By W. H. Longley

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THE conception that species have been multiplied by divergent evolution of related strains is based upon a great body of verifiable observations. Sound judgment has not been exercised consistently, however, in the attempt to establish the fact that their development has been directed throughout by natural selection.

If the Darwinian hypothesis is true, the characters of organisms should be largely of an adaptive sort, but its adherents have failed, upon the whole, to distinguish between shadow and substance, and have been content to support their position by *imputing* utility to structures and habits, when nothing less than rigorous proof of the fact will suffice. It is not demonstrated, for example, that any class of markings serves for purposes of recognition, or for signalling between individuals of one species. Neither is it proved that some color combinations warn off possible enemies, nor, indeed, that any type of pigmentation is functionally conspicuous. The last assumption, nevertheless, underlies a series of suggestions whose apparent conformity with its terms is held to support the hypothesis of natural selection.

Under the auspices of the Tortugas Marine Laboratory of the Carnegie Institution of Washington, I have been enabled to study many of the bright-hued fishes of the West Indian region, and have attempted to place the facts regarding their coloration upon an objective basis.

Countershading appears almost universally upon these animals. That is to say, their pigments which are externally visible are definitely graded from darkest on the mid-dorsal, or upper, to lightest on the mid-ventral, or lower line. Exceptions to the rule occur only among species of unusual habit or peculiar form.

The systems of pigmentation indicated is no immediate effect of exposure to light, although it involves the production of dark shades in any region in direct proportion to the average intensity of the illumination of that part. This is shown clearly by certain cases of sexual dimorphism in the color of crabs. In the *Portunidæ* the abdomen of the ovigerous female is exposed in dorsal view. In correlation with this family character the sexes differ in the coloration of that organ long before sexual maturity, when for the first time its position differs in the two. The abdomen of the male agrees in color with its sternum, but when that of the female is elevated it extends the pigment of the carapace posteriorly over the egg-mass and down into the shadow with delicate countershading. To explain this fact one may apparently appeal only to natural selection, or to sex-limited inheritance of the effect of exposure through many generations.

Other points of interest concern the striking color changes of thirteen species of fishes, which depend upon the color of the objects surrounding the animals. Further study will extend the list. These statements are based upon many records, of which a few were made under laboratory conditions. The greater number by far refer, however, to unconfined specimens studied from a boat, or from the bottom with diving equipment. The various phases of those which will gather about food provided for them may be induced at will by leading the creatures from place to place whose dominant colors differ. Changes of others obey the same laws and may be forecast with precision though they are not demonstrable with the same ease in uncontrolled individuals. Pictures taken with a submarine camera in ten feet of water record some of the observed changes, and show that their general effect is to reduce the conspicuousness of the animals that display them.

The distribution of colors among the various species examined has been investigated in a third phase of the research, and evident correlation of color with habit has been demonstrated. The results obtained may be expressed in brief as follows:

Those fishes in whose coloration red normally predominates are nocturnal, and, in proportion to their numbers, are rarely seen by day.

Gray appears with very different frequency among reef-ranging species and those whose diurnal activity is centered among the coral heads. The ratio of its occurrence in the two cases is roughly commensurate with that of its appearance in the environment of the contrasted groups. Among thirty-one reef-rangers, to state the facts in detail, fourteen show evident adaptive gray color phases or have permanent gray markings, while the same is true of only three of twenty-one species which remain near the coral heads by day. This is equivalent to saying that according to present information gray markings or color phases are about three times as common among reef-ranging fishes as they are among those which live close to the coral heads.

*Proceedings of The National Academy of Sciences.

Production of Smokeless and Odorless Flashlight Powders

FLASHLIGHT powders which burn without smoke or odor, and permit of photographing rapidly moving objects, are obtained by mixing finely-divided rare earth metals, such as zirconium, thorium, or titanium, with their nitrates or perchlorates.—German Patent to E. WEDEKIND and DR. G. KREBS.

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